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MIND
A QUARTERLY REVIEW
OF
PSYCHOLOGY AND PHILOSOPHY
EDITED BY
PROF. GILBERT RYLE

WITH THE CO-OPERATION OF PROF. SIR F. C. BARTLETT AND PROF. C. D. BROAD

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I.—INNOVATION AND PHILOSOPHY

By J. P. CORBETT

I

DISAGREE as they do about everything else, philosophers have commonly agreed in this, that a method can be found by which their disagreements will be ended. Descartes, in proposing a science which 'should contain the primary rudiments of human reason and . . . extend to the eliciting of true results in every subject'; Hume, in offering 'a complete system of the sciences, built on a foundation almost entirely new, and the only one on which they can stand with any security'; Kant, in promising 'to put metaphysics, in its first part, . . . on the secure path of a science'; Ayer, in maintaining that 'the questions with which philosophy is concerned are purely logical questions; and although people do in fact dispute about logical questions, such disputes are always unwarranted': all these philosophers, and many more, have agreed in deplored the uncertainty which besets our work, in believing that there is a remedy, and in supposing that they themselves, after all the mistakes and misfortunes of the past, have found it. But this has been the limit of their agreement. The remedies prescribed have been many and conflicting; even when two philosophers have purported to be following the same procedure, there has been no harmony in their results; and all the while the great stream of philosophical speculation goes rolling on, overrunning the dykes with which succeeding generations have attempted to confine its path and flattening the dams with which some have sought to bring it to a final halt.

Two courses are therefore open to us. On the one hand, we can try to build another lot of dykes and dams, hoping that in spite of all the disappointments of the past we may have the good

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I

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Two courses are therefore open to us. On the one hand, we can try to build another lot of dykes and dams, hoping that in spite of all the disappointments of the past we may have the good

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fortune to discover how this unruly flood of thought can be controlled ; or, on the other, we can call in question the idea, which underlies all such attempts, that there exists some means, as yet unknown, by which all philosophical disputes can finally be settled or dismissed. If we take the first course then, still believing that there is something wrong with the persistent uncertainty of philosophy, we must once more investigate the cause, hoping to discover, as all our ancestors have failed to do, a universal cure ; but if we take the second, then we must make a virtue of necessity, show that the controversiality of our subject is not a taint, but its peculiar merit ; explain why all attempts to end philosophical dispute have failed ; consider why they have so constantly been made ; and show good grounds for supposing that, in present circumstances, there is neither need nor likelihood of going through the same old farce in yet another form.

I take the second course.

II

I am by no means the first to do so. In a remarkable discussion,¹ from which, for an example, I quote at length, Mr. Hampshire observed of Moore's philosophical procedure that 'of course attempts have been made to freeze this style and practice into slogan and dogma—"Philosophy is only verbal analysis" or "Metaphysical problems are confusions in the use of ordinary language". But in these slogans the central suggestiveness and originality of Moore's work seems to me to have been missed : namely, the implication that in philosophy, just because it is not one of the sciences with assigned problems, we can do no better than unravel each perplexity as it presents itself, showing its unclassified oddness and trying to distinguish the various issues involved in it more clearly. If, in any particular case, we knew in advance what kind of problem is involved and to what methods it will yield, we would not be philosophically perplexed ; we would wait for the answer from formal logic or sociology or some other defined discipline. The latent suggestion is that there is no use and no hope in defining the limits and proper subject matter of philosophy or in looking for a general method of solving its problems ; secondly, that the whole programme, which has so long haunted philosophers, of finding a general method for eliminating confusion and logical error from our discourse is misdirected, and begins at the wrong end : that we cannot, and do not need, to do more than to try to make clearer, one by one,

¹ 'Changing Methods in Philosophy', *Philosophy*, 1951.

the sources of the metaphysical puzzles which present themselves as our forms of thought and language change : that—to adapt a phrase of F. P. Ramsey's—we waste time and mislead ourselves if we try to make anything or everything absolutely clear, as opposed to making several distinctions clearer ; for to try to make any distinction absolutely clear is to try to circumscribe the development of language, and the use of this expression within it ; and to try to make all the permissible uses of language clear is to try to insist that all knowledge must conform to some predetermined pattern.' But 'Why should we not now finally accept that philosophy itself must always be experimental, and without predetermined limits or anticipated problems ? Why should not philosophers use any procedure of analysis which in particular contexts and at particular times seems in fact to bring greater clarity, without stopping to convert these *ad hoc* procedures into general methods or to make universal claims for them ? It is just these universal claims and final definitions of the true nature and limits of philosophy which have always proved confusing and restrictive in the long run. I suggest that what is new and genuinely original in contemporary philosophy, or in the best of it, is just the fact that it offers not yet another new method or system, but (almost for the first time) a cultivated absence of method or system, an implied acknowledgment that every advance towards greater clarity about any feature or argument must always be partial, provisional and never final ; a changing miscellany of questions, all attempts to find an underlying common origin of philosophical problems seeming only the ghost of the old fallacy that philosophy must have some unique subject matter of its own—if not Being or Reality, then Language !'

This is, at least, an interesting view. It merits exploration. Since all attempts to make philosophy respectable have failed, why not let it for a moment be disreputable instead ? Why not let it be experimental in its methods and tentative in its results ? And then perhaps we may discover that in judging it to be disreputable we have been setting it a standard to which it cannot now conform in fact, and to which it can only be made to conform in appearance at the price of unacceptable distortions.

III

What then counts against the view that philosophy requires a strict and universal method, yielding a comprehensive and definitive system ?—Science, or, more precisely, the idea of science which its recent history has imposed upon us.

Descartes' idea of the philosophy of science was of a body of doctrine which consisted, first, of a number of propositions about the existence and properties of God and of the soul ; second, of certain considerations designed to establish the truth of those prime metaphysical assertions ; and third, of demonstrations that, given those prime assertions, certain other propositions followed concerning the existence, properties and relations of material things. These last propositions—the law of inertia, for example—were thus supposed to be established *a priori* and to suffice thereafter as the basis for the explanation of all possible phenomena.

This Cartesian argument can no doubt be attacked directly. Analysis can discover obscurities and inconsistencies within its structure. Equally without doubt, such obscurities and inconsistencies could be made good, if anybody wished to do so. But what is really fatal to this view has been the subsequent development of science. It was a necessary consequence of Descartes' philosophical procedure that the framework of physical principles which he derived from metaphysics should be universal, in the sense that all phenomena could be lodged within it ; necessary, as being sanctioned not ultimately by observation, but immediately by God ; and therefore permanent, in that no situations could possibly arise as the result of which it would have to be qualified, corrected or extended. There was indeed much to be done before the framework could be said to have been accurately applied to fact at all important points ; and the *Discourse* was published to enlist the necessary help from others for the achievement of that end ; but the help that he looked for was merely help in application. He, Descartes, all by himself, in a few years of earnest meditation, had reared a fabric of ideas which could contain all future science. If any part of it proved false, he said, the whole of it would be worthless.

All logical weaknesses put aside, this argument strikes us now as simply silly. And that is not just because Descartes' conclusions on specific points of science turned out to be wrong, as with the velocity of light ; nor just because whole sections of his mechanics are fallacious, as with his theory of impact ; nor just because the course of science has overflowed at every point the conceptual banks within which he and his fellow mechanists attempted to confine it. The difference between Descartes and ourselves is still simpler and still more fundamental. For us, the business of the scientist is not, as Descartes thought, to elaborate a set of principles explaining everything, for ever ; it is to elaborate one set of principles after another, each one of which

explains a wider range of fact than did its predecessor, opens new doors to new empirical investigations, and so prepares its own eventual supersession. Science is, for us, the permanent revolution of all the ideas and principles in terms of which we order, interpret and explain the world. The scientific genius is not the man—or superman—who brings this process to an end ; he is the man who speeds its course while knowing that the work by which he does so will in its turn itself be overrun by that of others.

This idea of science as a permanent intellectual revolution, though it has long been practised, has only recently been clearly recognised and preached. Strangely enough, it seems to have been Newton's great success in practising this principle which prevented so many of his successors from seeing it for what it was. Newton's remarks about ' hypotheses ' (in his unfavourable sense), and about the great ocean of truth which lay all undiscovered before him, showed his own clear grasp of the real nature of the scientific process ; but the fact that his mechanics proved such an effective instrument of explanation for so many phenomena for so long a time led many people to an opposite conclusion. Thus, for example, Kant, in attempting to derive the main concepts and laws of Newtonian mechanics from the very nature of human reason, was only expressing in a particularly plain and rigorous form what was quite generally believed until the present century : that Newton had laid bare the ultimate structure of the universe and therefore had succeeded, where Descartes had failed, in establishing the necessary framework of every proper explanation. But we are differently placed. During the last hundred years, the pace, the depth and the range of the permanent scientific revolution have all become so great that we cannot seriously think of science in those stable terms which, though even then illusory, were plausible two hundred years ago. The permanent revolution of ideas has become a main part of modern common sense.

That being so, we have an overwhelming reason, at least as far as the philosophy of science is concerned, for having no more truck with further methodological ' revolutions ' designed to endow philosophy with a procedure by which its problems can be comprehensively set out and definitely solved. For the problems of the philosophy of science can be seen to arise in fact from scientific innovation ; and scientific innovation is unpredictable and endless. Neither those problems, nor therefore the methods appropriate to their solution, can possibly be predetermined. We can indeed say something platitudinous to the effect that the problems of the philosophy of science are those theoretical

problems raised by science which are not amenable to empirical tests ; but that is merely to mark out a territory, not to say what lies within it, nor how it is to be traversed. Nobody, in the nature of the case, could have foreseen the philosophical problems that would be raised by the axiomatisation of mathematics, by the postulation of the unconscious in psychology or by quantum mechanics until those conceptual situations had actually been brought about ; and nobody could therefore have laid down in advance the procedures by which the conceptual conflicts to which they have given rise might be resolved.

The fact of the matter is that to lay down and adhere to any procedure in the philosophy of science is to be committed, in the circumstances of permanent scientific revolution, to one of two intolerable positions. Either, like Descartes, you must try to halt the march of science, fixing it within the form which squares with your procedure ; or like 'pure' metaphysicians and their twins, the votaries of common language, you must detach yourself from science altogether. But both of these positions are absurd. Both compel you to neglect the thing that you are endeavouring to understand. Granted therefore that science is nothing if it is not the permanent revolution of all our thought about the world, then, at least as far as the philosophy of science is concerned, we must agree with Hampshire 'that there is no use and no hope in defining the limits and proper subject matter of philosophy or in looking for a general method of solving its problems'. It is not merely that we can see in fact the failure of Descartes' attempt to do philosophy in the manner of mathematics, or of Hume's attempt to do it in the manner of introspective psychology, or of modern attempts to do it in the manner of formal logic or of grammar. We can see in principle why every such attempt has failed in the past and will fail in the future, so long as science may continue. They try to elucidate a changing situation in an unchanging way. Their method contradicts their purpose.

IV

There is more to philosophy than the philosophy of science. We are as much concerned with moral and with social issues. What is the situation here ?

The permanent revolutionariness of science is not, as the words of some writers would suggest, a kind of spiritual force, blowing whither it listeth, like the wind, and sweeping society along whatever course it may independently determine. It depends on social causes, is related to social aims and operates through

social institutions. But to follow these connexions out is simultaneously to discover that the conclusion that we have had to draw for the philosophy of science must next be drawn for the rest of philosophy as well.

It is only within the last hundred years that the permanent revolutionariness of science has been effectively established, in the only way in which it can, by the growth of institutions devised specifically for the purpose. It is true that the Royal Society and similar bodies are almost as old as modern science itself and that, from the start, they gave it much of its peculiar strength; but yet, until quite recent times, the pursuit of science was still really left to the unsponsored initiative of individuals. Recruitment into science depended on the chances of individual interest; and scientific work depended on the resources of the private purse. It was only when science lodged itself simultaneously within the universities, within industry and within the state, when schooling was reformed to provide abundant recruits for these new institutions, and when ample resources for research were forthcoming from public and industrial funds, that the revolutionariness of science was made permanent by being institutionalised. This has already occurred in western countries and will in due course occur, as is now plain, throughout the world. In all industrialised societies a quantity of pressures, as complex as they are powerful, ensure a large supply of able individuals for whom ceaseless innovation in all our ways of looking at the world is the proper and habitual mode of life. But why has this occurred?

Most if not all of the philosophers of modern science had their eyes upon the practical advantages which it would bring to man. The science of which Descartes dreamed was to make us the 'masters and possessors' of nature; and the whole Cartesian tragedy lay in the conflict between that practical hope and the demand for *a priori* certainty in science which flatly contradicts it. In this conflict it was, in the world though not in Descartes' thought, the practical hope that won. From the dawn of civilisation human conscience has rebelled against the scantiness of the means of life and against the unequal division of what means there were; and, from the moment that the new intellectual skills of science made it possible for those means to be indefinitely increased, the line of the future was assured. The first steps of the industrial revolution were slow, halting and unpleasant; but yet the advance was sure, for it was only in that way that the deepest tensions of society, first formulated by the Hebrew prophets in their demand for an egalitarian, just and bountiful

society, and persisting¹ through centuries of inequality, poverty and oppression, could be resolved. If therefore the scientific revolution was made permanent within society by the growth of institutions specifically adapted to that end, this was not due solely to a disinterested love of truth, though that also is a very powerful motive, but to the realisation that in that way alone could the intensest demands of society, the demands for welfare and equality, be satisfied. Corresponding, therefore, to the institutions which produce the permanent revolution of our ideas about the world, and supporting them at every point, there now exist institutions which produce the permanent revolution of the means of life. Not economic institutions only but the whole political machine is now firmly geared to the production of rapid, deep and comprehensive social change. Governments are now committed to maintaining, in the longer run at least, such a high level of employment, investment and research as will ensure a rapid increase in the standard, and therefore a rapid alteration in the whole pattern, of the life of their society. This massive social innovation may not have been the aim of those who set the process going. They may have thought more simply of the specific ills of want or of disease which could and should be cured. But it has proved to be in fact the case that the means to which alone those ills would yield—the means of industrialised society directed by the state—have drawn us inevitably into endless social innovation ; and now that that has happened, people have come to like it for its own sake. It has become an established part of common sense not only that we shall think but also that we shall live quite differently in a few years time ; and also that we shall be all the better for it. We live in a world of practical as well as theoretical innovation ; and the two processes are indissociably connected not only with each other but with all the prevailing trends and pressures of the time.

Now our philosophical reflection upon human conduct and the social order does not take place within a void. It takes place within the context of our intellectual and practical powers, whether individual or collective. It is these powers, and the conflicts in which their exercise results, which set us our first order moral and social problems. As these powers therefore change, so do the practical problems with which we, as individuals and as citizens, have to deal, and so also do the philosophical issues to which these practical facts give rise. In fact, the material of the moral and social philosopher changes, as much as does the material of the philosopher of science. But the nature of the

¹ Cf. Cohn, *The Pursuit of the Millennium* (London, 1957).

change involved is still more intricate. The work of the philosopher of conduct centres around such terms as 'responsibility', 'virtue', 'liberty' and 'state', just as the work of the philosopher of science centres around such terms as 'law', 'explanation', 'space' and 'cause'. But the first of these two classes of terms plays a double role in language which the second class does not. Terms like 'state' and 'virtue' are used *in practice* as well as in the theoretical analyses we make of *practice*. To put the same point in a different way: conscious activities, and therefore language, are part of the subject matter of psychology, history and the social sciences, as they are not of the sciences of nature. It follows that in a systematically innovating society such as ours the philosopher of conduct has to deal with the deeper conceptual consequences of two distinct though closely related kinds of change. On the one hand, our individual and collective conduct, our attitudes, and the linguistic forms through which these work, are themselves always changing. People cease, perhaps, to be troubled by a sense of sin, and cease therefore to engage in the activities which have been undertaken to appease it; or the state assumes a whole new range of economic functions which have been previously excluded from its view. But as our lives change in such a way, so the forms of language through which we organise them change as well. 'Virtue', 'responsibility', 'liberty' and 'state' come to assume quite different forces and so to present the philosopher of conduct with novel problems. The practical use of advances in endocrinology transform our notions of responsibility; new techniques of mass persuasion transform our notions of political freedom; and each of these transformations raises in its turn a novel set of philosophical issues. But while these practical changes are going on, the psychologist, the historian, the sociologist, and the economist are at work, permanently revolutionising, like the natural scientist, all our thought about our individual and collective conduct, whether this conduct is, or is thought to be, changing, or whether it is not. But the permanent revolution of our ideas about our conduct raises philosophical problems on its own account just as much as does the permanent revolution of our ideas about the phenomena of nature. As the technique of economics changes, so, for example, are we compelled to ask, and ask again, what economic explanation is, and how it now differs from explanation in physics and in history. Finally, these two kinds of change, the practical and the theoretical, are intimately interlinked, both logically and causally. New theoretical insights transform our conduct, while the necessity of novel actions calls forth the novel

insights which they presuppose. The work of Keynes strikingly illustrates, as he himself well knew, both of these patterns of connexion.

To work out in some detail these interactions between our practical and theoretical uses of language, charting the immensely intricate area in which they overlap, is a main task of the philosophy of conduct at any time. But here, where it cannot be taken any further, all that is needed are the fundamental points that the material of the philosophy of conduct is always changing, in this very complex way, in any systematically innovating society such as ours ; and that the pattern of this change is unpredictable, first because we are not able to foresee the course of discovery in science, and second because, even if we could, we would not be able to foresee its social repercussions. It follows that, situated as we are, the desire for a philosophical procedure which would produce definitive results is as inept in relation to the issues of morality and politics as it is in relation to the issues of science. To act on this desire would be, once more, to cut ourselves off from the very thing that we are endeavouring to understand : the constantly changing, deep conceptual issues in which we and our society find ourselves involved as the actual organisation of our existence systematically changes shape. Situated as we are, there is no room left anywhere for the kind of philosophising which Hampshire urged us to reject.

V

Most people will no doubt agree that it is quite absurd, inasmuch as it is quite impracticable, for a philosopher to attempt, by the implication of his method, either to arrest or to neglect the course of science and society. He cannot hope to do the one and cannot afford to do the other. But to say that such intellectual policies are absurd is probably to mean more than that. It is to mean not merely that they are impracticable, but also that they are wrong. Most people nowadays believe that science and social change are good, though occasionally dangerous : good both in themselves and also for what they can bestow upon mankind. It is this belief which ultimately underlies our hostility to any social forces, as well as to any methods of philosophy, which work against the permanent revolution of our ideas about the world and of our actions in it.

But now let us postulate, instead of people who, as we do, hold to that belief, a person, a class or a society who thinks that only theological knowledge is of real importance, while enquiry into

nature is a peril to the soul ; and who is quite satisfied with the order and resources of society as they are, feeling that the attempt to change them is a challenge to God's original ordination of the world. There is nothing impossible about these attitudes. They have been widely held ; they are still held in certain circles ; and a complete, consistent set of intellectual, moral and political positions can be worked out in accordance with them. But given such positions in the place of those we have, our judgment of what philosophical procedures are sensible and what absurd would be reversed. In arguing against the general procedure of most earlier philosophers, Hampshire pointed out, what is unchallengeable on any view, that they 'try to circumscribe the development of language (and) to insist that knowledge must always conform to some predetermined pattern'. He thus assumed that the undoubted fact that those procedures do tend to circumscribe and predetermine knowledge will serve to demonstrate that they are wrong. This argument, however, obviously assumes that the circumscribing and predetermining are wrong themselves. But in what sense ? If you believe that life and knowledge, as they are, have been ordained by God, as many have and some do still believe, then the attempt to set them out and lay them down—to circumscribe and predetermine them, in Hampshire's hostile terms—cannot be wrong in any sense. Rather it is, perhaps, an act of piety, registering the natural law which God has laid upon the world ; an act of prudence, abjuring the promethean urge for natural power ; an act of wisdom, yielding a deeper insight into things than can be won by any restless probing after natural causes. Therefore the true philosophical impulse is towards some framework of ideas within which all experience can be completely, clearly and permanently lodged ; the true philosophical procedure will be one which yields such rigorous results ; and the true philosopher, at any time, must seek the cause why such results have only been partially achieved, and then must find the cure. If the value premises of social permanence are accepted, this attitude towards philosophy is the only one which can be rationally held ; and, if we hold it, no purely methodological argument, like that of Hampshire, will be able to dislodge us.

Thus the choice, with which Hampshire rightly faced us, between, on the one hand, seeking out a method which—at last—would make philosophy 'scientific' and, on the other, cultivating 'an absence of method or system', is indissociably connected with a general choice of values. The desire for permanence, with all its consequences, pulls you one way, while the desire for change,

with all its consequences, pulls you the other. You cannot support the claims of an unsystematic as against a systematic method of philosophising simply by appeal to common sense, or to the facts of science or society : for the commonsense of other people has been and is quite different from our own ; and the fact that science and society work in the ways they do is not sufficient to establish that they ought to do so. In this matter, we do not only stand at the parting of two ways of philosophising ; we stand at the parting of two ways of life.

And this, in a sense, is nothing new. All modern philosophers have been in this position to the extent that they have all been faced with the permanent revolutionariness of modern science and society and have all sought to incorporate it, somehow, into their inherited conceptual world. Indeed, the irony not only of Descartes' but of most of his successors' work has been precisely this, that while accepting systematically changeful science, with all its social implications, and while therefore seeking to understand innovation and to include it in their vision of the human situation, yet their deepest methodological assumptions have systematically prevented them from achieving that result. Even Marx, who came nearest to an understanding of the point, and who laid such emphasis on change, was still enough of an Hegelian to think it proper, necessary and fruitful, if not to terminate philosophising in a system, yet to build up long conceptual dykes within which only, so he argued, could the philosopher interpret and thus change the world. As much therefore as the philosophers whom he rightly criticised, Marx was committed to misunderstanding scientific and social innovation and to excluding it from his scheme of life. For you cannot understand the systematic innovation which is the new basis of existence until you accept it as a process by which every concept, every principle, every practice and every attitude is affected in the end, and in ways which cannot be predicted or laid down ; and you cannot accept innovation in this sense so long as your philosophical procedure commits you not to elucidating what is happening, here and now, to the constantly changing abstract framework of our thought, but to establishing what the framework of that thought *must be*, and therefore permanently *is*. But why did our philosophical predecessors remain within this contradiction ? Since they particularly wanted to account for science, why did they not reject that demand for philosophical finality which made it impossible for them to do so ?

There seem to have been four main reasons why it has taken philosophers so long to get in methodological step with the

acceptance, in principle, of innovation. First, it has taken time for scientific procedures in the modern sense to spread from physics, where they scored their first unmistakable success, over the whole field of thought about the world. In particular, the attempt to apply them rigorously to the phenomena of the mind and of society is an affair of the last fifty years and still is only on the threshold of unmistakable success. Meanwhile, in those fields of fact which scientific procedures had not yet penetrated, unscientific procedures had necessarily to continue. Philosophers themselves necessarily continued them. It is therefore not surprising that, since so much of their work belonged to a different epoch of human thought, their understanding of the scientific revolution was incomplete. Second, as we have seen, it is only within the last hundred years that the permanent revolutionariness of science has been manifestly established, in the only way it can, by the invention of institutions devised specifically for that purpose. Third, it is only within the last decades that our political and economic institutions have so developed that their whole force is manifestly thrown into the production of the social change which science constantly makes possible. And fourth, while this long immaturity of innovation made it possible for philosophers to neglect the revolutionary nature of the situation in which they really were, certain extremely powerful forces encouraged them to do so. Most notably the belief in an absolute, and therefore permanent, scientific, moral and political truth, and therefore in a philosophical method appropriate to its determination, had, in the circumstances of earlier times, got so involved with religious practice that the two things appeared to the majority of thinkers as being strictly indissociable. There could be no religion without the acceptance of such truths, and no acceptance of such truths without religion. Hence, until this old association could be broken down—and this has only just begun to happen, and still is very incomplete—the whole force of religion sustained that old idea of the structure of the intellectual world which innovation threatened and which it has now to all intents and purposes demolished.

Thus modern philosophy has been driven more and more strongly in one constant direction for some 300 years by the rising tide of innovation, and yet has been constantly prevented from recognising and accepting the nature of its case, in part by the long immaturity of innovation, in part by old connexions and allegiances, like those that centre on religion. Modern philosophy has therefore moved from one compromise into another, always trying to do justice to its driving force of innovation while

always seeking to preserve some element of its original claim to end all our uncertainty about the foundations of our thought, and borrowing for the purpose now this, now that procedure which had been found to produce reliable results in some particular field. But it is now clear that the days of compromise are over. The forces of innovation are now so strong and manifest that they can no longer be neglected or concealed. Now we are obliged to make an open choice, either to act and think with innovation, or to act and think against it. But then, what kind of choice is this? And, if we opt for innovation, what can be said more positively about the new kind of philosophical procedure to which we shall thereby be committed?

VI

That we have some kind of choice, we have already seen. Instead of accepting innovation, we can take our stand upon the permanence of a certain social order, justifying our decision by the claim that certain moral, social and scientific views—those of St. Thomas, for example—which are supposed to be embodied in it, are absolutely true. Or, while apparently accepting innovation, we can really take our stand upon the permanence of a certain line and instrument of change—the proletarian revolution and the communist party, for example—and justify our decision by the alleged absolute truth of a certain scheme of history from which their inevitability is said to follow. And then, if we take either of these stands, our position can easily be made impregnable by building into it a methodological device excluding all possible criticisms before they have been made. A doctrine of divine revelation, or of intuition, or of dialectic, can always ensure that every criticism is debarred *a priori* by the personal incapacity of the critic. Then, well-fortified by doctrine, we may proceed to employ whatever means may come to hand, from education to depth-psychology,¹ and from fire to gas, to make our fellows live within the permanent social order, or the prefigured line of social change, to which we have given our assent.

Plain alternatives to innovation therefore do exist; and if they are resolutely held they are impregnable to theory. For while theory can only point out apparent inner contradictions, these alternatives contain a clause interpreting all apparent contradictions as failures to understand. But then, if that is so, the 'choice' between these two alternatives, or between both of them and pragmatic innovation, seems not to be a choice at all. For in

¹ Cf. Vance Packard, *The Hidden Persuaders* (London, 1957).

order that the following of one line of action rather than another can be accepted by all parties as the result of choice it is necessary that reasons can be given for it which, while not accepted by those who disagree with what is done, are not excluded by them as absurd. But this, precisely, is what happens here. We seem therefore to be confronted, in the present case, with examples of those total breaks in human thought across which, in the nature of the case, there can be no communication ; and the actual futility of all 'discussions' between Catholics, Marxists and the remainder of the world confirms by experience this point of theory. What we face here, it seems, is not a choice at all ; it is a blind jump.

It does not, however, prove to be in fact the case that these divisions of ideas are quite as absolute as this analysis suggests. The position of those who are opposed to innovation is only impregnable and beyond discussion so long as it is kept consistent, so long, that is, as all kinds of innovation are rejected in theory and blocked in practice. If, for example, science is exempted from the ban, then not only has one activity been admitted which cannot possibly, as we have seen, be integrated into the metaphysical scheme of thought, but also, as experience shows, amounts and kinds of knowledge will eventually be released into society by science which will make the task impossible of keeping its structure, and so the moral and social principles which it embodies, unaffected. When science is pouring out new kinds of knowledge, and therefore making possible new forms of life, it seems that no institution can be strong enough to keep society from moving off in new directions, however destructive to the system they may be. Hence the opponents of innovation must oppose *all* innovation or else they will find themselves to be both theoretically and practically inconsistent. Compromises, such as Soviet biology, have of course been tried ; but these attempts to harness and to blinker science always fail, since either the science is destroyed or the harness and blinkers have to be discarded. But to oppose *all* innovation is much more than the great majority of those who are committed to so doing by the logic of their system can really stomach. The value of scientific, if of no other kind of innovation, is admitted by them all, both for itself and for some at least of its practical advantages. (All religious systems, for example, are committed to caring for the sick.) The position of such people is therefore inconsistent, as our argument has shown ; and it is this inconsistency, this final inability in practice to reject all innovation, and therefore this ultimate commitment to innovation in its entirety, which make discussion possible between ostensibly conflicting ideologies today.

The real choice therefore is not between acceptance of innovation and its flat rejection, but between a confused and partial acceptance and one which is complete and clear. It is the fundamental task of philosophers at the present time to work from the first towards the second. But in this task empiricists have a particularly important part to play. For while, methodologically speaking, the record of empiricists has been almost as equivocal as that of their opponents, for the reasons we have seen, the acceptance of innovations has always been their ultimate objective. They may in fact have tended to petrify thought and language by assimilating the method of philosophy to that of psychology, like Hume, or, like more recent thinkers, to that of formal logic or of grammar; but yet their aim has always been to speak about and on behalf of science. It is therefore much easier for them than for a thomist or a marxist to take the final step that Hampshire recommends and so to be the first to realise, in a clear, precise and comprehensive way, the full nature of the intellectual revolution which the establishment of innovation is imposing on mankind. And, having realised and accepted this revolution for themselves, they can take the lead in so working on the inconsistency which we have detected within all systems of natural law and natural progress as to promote in other people the new design of intellectual order which is implied in innovation.

It is high time that this should happen. It is now all too plain that the latest fruits of scientific and technological innovation are intolerably powerful weapons in the hands of those whose minds are fixed in one or other kind of social dogma; and philosophers, if they cannot hope to master innovation and control its fruits, can yet do something to release the mind from these disastrous cramps.

VII

What then can we say more positively about the new kind of philosophical procedure to which we are committed by this programme?

However much we may disagree with our great predecessors about the hope of bringing logical error and confusion to an end, we must certainly agree with them that it is those facts about our thought which set philosophy its problem. Human enquiry finds itself always, willy-nilly, beset with contradiction and confusion. This is a fact, whether or not we like it, and whether or not we think we can explain it. But, of course, it is not every kind of contradiction and confusion with which it is the business of

philosophers to deal. A contradiction between existing physical theory and some newly observed phenomenon is the business of the physicist; a confusion in the way that a tract of history has been divided is the business of the historian. The philosopher's contradictions and confusions are of a special kind. Appearing in and out of the many different vocabularies by which we interpret and attempt to change the world is a relatively quite small number of terms which play a peculiarly important role in the organisation of enquiry but which are much easier to enumerate than they are to classify or to describe. Such terms are: 'explanation', 'cause', 'event', 'necessity', 'value', 'freedom', 'progress', 'revolution' and the like. These terms not only provide an indispensable logical skeleton on which the flesh and blood of each particular enquiry is arranged, but also, running from one enquiry to another, bind them together into a certain whole of thought. It is the nature of this whole of thought which is at stake in the procedural dilemma which Hampshire set before us. Both sides admit that, as things are, this whole is confused, and broken up by contradictions. We cannot think at all without making use of these logically deepest and most extensive terms; but, as we do so, we find that our uses of them are in fact confused and inconsistent, both in themselves, and as between each other. Thus many different and conflicting uses of 'explanation' jostle darkly with each other as we investigate the world, and some of them, when applied to human actions, apparently preclude us from considering those actions, in the way we do and must, as valuable and free. It is these peculiarly fundamental conflicts and confusions, distinct from, though intimately connected with, those which arise in the course of a particular enquiry but whose implications do not reach beyond it, with which the philosopher is professionally concerned. For he is blessed—or cursed—with a strangely sensitive logical conscience. While other people, seldom letting their attention rest upon these conflicts, get on with the affairs of life, the philosopher is fascinated by them. He cannot forget them, cannot look away. But then, to repeat the question, if he abandons the belief that with sufficient diligence and perspicacity he can eliminate them all, bequeathing to later generations a clear, consistent, comprehensive mansion of ideas in which they can live happily for evermore, how is he to undertake his work, and in what terms is he to think about it?

With these questions, we have come to the decisive point. Can there be another inspiration for philosophers besides that metaphysical urge to transcend, in order then to circumscribe and

predetermine, the growth of thought and language? Can there be an inspiration beyond that urge which we have found to be flatly inconsistent with our innovating form of life? What else can we philosophers look for, besides an ideal conceptual structure of total clarity and rigour?

Now although it is true that the philosopher alone is so absorbed by the deepest contradictions of our thought as to be compelled to give his whole attention to them, yet it is also true that these same contradictions intermittently trouble everyone who gives serious thought to the particular problems of theory or of action. Conflicting ideas of explanation do trouble those who have to give them, as witness, for example, the persistent debates between economists, affecting the whole course of their procedure, as to the nature of their goal. The conflict between causality and freedom underlies and persistently disturbs debate about the reform of penal systems.¹ The contradiction between the deontological and the teleological approach to ethics enters directly into all discussions of such a topic as divorce.² The confusions which normally lie hid within a term such as 'equality' are forced into the centre of affairs when a political party is led by electoral reverse to reconsider all its attitudes. Thus, while the philosopher may be particularly sensitive to contradiction, he is not abnormal: he only feels all the time those logical strains and stresses which other people feel upon occasion. But upon what occasion? The above examples, taken at random, suggest an answer which others will confirm. It is when we are *dissatisfied* with some procedure of enquiry, when we wish to *reform* some item of the law, when we must find a *new* programme of political action, when, in a word, we are constrained to *innovate* that we all find ourselves involved in those profound conceptual tangles with which it is the business of the philosopher to so deal as to reduce them to a form in which they can be effectively handled by scientists and practical men. Thus innovation does not just destroy the old manner of philosophising: it establishes the new. To find our novel inspiration we have only to reflect that as well as being philosophers we are people, and not people in the abstract, but people living here and now, as members of a particular society in a particular stage of its development, and people who therefore are subjected, more or less directly, to all the transient and enduring conflicts which make up its life. We do not have to invent conceptual puzzles, any more than we have to find them in the eternal nature of things: they are forced upon us by events,

¹ Cf. Royal Commission on Capital Punishment (1949-53), p. 99 ff.

² Cf. Macgregor, *Divorce in England* (London, 1957).

and the first wisdom of the philosopher is to recognise that this is so, to abandon spurious claims to being busy with technical problems of his own above the battles of the world and to accept the task of sorting out, in whatever way gives promise of success, those particular confusions and contradictions in which the thought and practice of his time are actually involved, and by which they are actually inhibited.

But once the social immanence of philosophy has been accepted, once, that is, it has been seen that philosophy, like everything else, is dominated by the universal tide of innovation, then it immediately becomes obvious why our work must have the properties ascribed to it by Hampshire. Philosophising, like innovation itself, must be piecemeal, for we cannot innovate everywhere at once and must take some things for granted while we transform the rest. It must be controversial, reflecting in its abstract mirror the breaks, the divisions and the conflicts through which innovation always must proceed. Its results and methods must be transient, like its problems, for while it is true that some of these have longer lives than others, yet none of them are quite invariant to change, and all of them take on novel shapes, and so need to be reconsidered, with the course of time. And its course must not only be interminable but unpredictable, since it is impossible in principle to foretell the future course of science and society,¹ and impossible therefore to foretell the future problems, or the future form of problems, with which the philosopher will have to deal.

It follows from the present thesis that little more than this can profitably be said, in general terms, about the method of philosophy. The crucial point is this, that we philosophers, like everybody else, have got to face the facts of systematic change. We must accept it, or we must oppose it; for to dither is to deprive our work of point, condemning it to be the mere history of previous philosophising or the mere splitting of conceptual hairs. We have to choose between two forms of life, of which the dogmatic and pragmatic procedures which we have been considering are the philosophical components. If you oppose it, then you must explain how you can do so, in practice as well as theory, consistently and with success; but if you think that systematic innovation is to be accepted, so that you see an endless epoch of theoretical and practical innovation opening out before mankind, and wish to take your place as a philosopher within it, then you must view your work and set about it in quite a different way. You will not think that there are philosophical problems

¹ Cf. Popper, *The Poverty of Historicism* (London, 1957), Preface.

which can be specified independently of problems of any other kind, and which are therefore invariant to changes in the rest of thought. Consequently, you will not be tempted to suppose that there is some procedure to be found by scrupulously following which the philosopher's problems would be disposed of in a regular and decisive way ; and you will not be disappointed when that fails to happen. Instead, while giving occasional thought to method, criticising old procedures and suggesting new, in order, like any other thinker, to concert your approach to problems and to explain to others the way in which you do so, you will realise that in the nature of the case there can be nothing permanent or decisive in the procedures which you in fact adopt, in the formulations which you give them, or in the results which you thereby achieve.

Thus, from the moment that innovation has been accepted as the form of life, there seems to be only one possible set of general philosophical imperatives : seek out the deepest contradictions and inhibiting confusions within the current movements of the world ; do not try to stay on the heights of 'pure' philosophy, where the air is so rarefied that it cannot be breathed ; get down into the stuff of change ; find where the forms of language are inhibiting the flow of novel thought and action ; employ whatever means seem likely to release those jams of understanding ; admit that in the nature of the case there can be nothing permanent in your aims, in your methods or in your results ; at the level of those fundamental abstractions upon which all the order of our thought depends, seek out, not the permanent fixation of ideas, but their permanent revolution !

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II.—BIRTH, SUICIDE AND THE DOCTRINE OF CREATION: AN EXPLORATION OF ANALOGIES

BY WILLIAM H. POTEAT

Prima facie, nothing would seem to be more unlikely to clarify the peculiar nature of certain concepts in theological discourse than an analysis of the expression ‘I was born’ and an examination of the nature of a decision to take one’s own life. Nevertheless, I believe that such an inquiry will be of value in explicating certain features of the language of “beginnings” which is a familiar part of theological discussion, and one which has perhaps presented peculiar difficulty in the one hundred years since the publication of Darwin’s *Origin of Species*.

One of the many things that Christians profess to believe about God is that he is “maker of heaven and earth”—where this is taken to mean that before the divine act, through the utterance of God’s Word whereby the world with which we have to do is thought to have become what it is, there was nothing. Hence the so-called doctrine of *creatio ex nihilo*. This view, it is supposed, is a characteristic of both Christian and Jewish belief which sets them apart from the beliefs of all religions—such as Hinduism, Buddhism, etc.—which are either explicitly or implicitly a-theistic, and also against metaphysical systems where either there is no God, in the theistic sense, or where, if there is, he is thought to be no more than an artificer, working upon some antecedently given matter.

Along with this belief, it is also asserted that man is made in the image of God. Let me, as preliminary, take this to mean, so far as the present inquiry is concerned, that man, himself a creature, stands to the created world (understood as the subject of our public, common sense, or even our scientific curiosity) in a way analogous to that in which those who believe in *creatio ex nihilo* suppose God to stand to this world.

Now, it is notoriously difficult to assimilate logically what is thought to be meant by *creatio ex nihilo* to the many other things that we say about the world (whether we interpret the concept ‘world’ here in the Kantian sense as a regulative principle for *cosmologia rationalis*, or merely as any given finite sum of synthetic propositions about phenomena that may be thought of as being “in” the world, taken in the Kantian sense). Theologians have

declared the notion to be a mystery,¹ and philosophers, beginning in the modern period with Kant's antinomies, have generally regarded it as having no definite meaning.

I propose to show that though *creatio ex nihilo* is indeed a queer conception, which leads theologians to speak of mysteries and philosophers to speak of nonsense, in fact the notion is not so remote as has been supposed from certain demands within what is nowadays called our ordinary ways of speaking; and that within these ordinary ways of speaking where we are talking in a logically extended way about matters which are both meaningful and important to us as persons, there are displayed analogies with the sort of thing the theologian has in mind when he uses 'creation' and 'image of God'. To anticipate later argument, I think it can be shown by an analysis of some of the things we normally say and think of ourselves as persons that what we mean when we say them is logically heterogeneous with certain other things we say about ourselves, and therefore they may be said to be both "queer" (if we take as our paradigm for what is "unqueer" these other things we say) and yet meaningful; and that in them some analogy with what one might mean by *creatio ex nihilo* is to be found.

While, of course, this does not necessarily accredit the use of analogies drawn from these tracts of our ordinary ways of speaking for describing God; and while it certainly does not authorize the use of theological language in general; it does give us some genuine insight into what it is that the Christian might mean when he uses them.

This, I propose to do by exploring suicide as in some sense an act of absolute and radical destruction; and by analyzing certain features of the expression 'I was born'.

I

The impulse to see man as in the image of God, and especially to see that image manifest paradigmatically in a dark and violent gesture of defiance, destructiveness and nihilism is of such antiquity that we cannot but wonder what kind of posture man is thought to achieve in relation to the world and to himself in these

¹ " . . . In the history of religion the idea of real creation first appears when God, instead of being considered a merely natural force, becomes a transcendent Being. We do not know this God through experience or reason but through faith, and we know of the mystery of creation by the same means." E. Frank, *Philosophical Understanding and Religious Truth* (Oxford Univ. Press, New York, 1945), p. 58.

acts in which it seems so natural to see the image of God. Adam and Eve are promised that, if they will eat of the tree of the Knowledge of Good and Evil, they will become as God. While this is a calamitous act, it is nevertheless one in which man is felt to exhibit a real if perverted likeness to God. Man's act of rebellion is in certain respects logically like God's act of creation! The logical parallel between God's act of creation and man's act of destruction is clearly assumed. If this be so, it is necessary to explore some of the features of the way we think of ourselves that are built in to our talk about ourselves as persons.

St. Augustine, in his *Confessions*, reflecting in later life upon a boyhood act of wantonly stealing pears which he did not want and could not possibly eat, and concluding that the only possible answer to the question "Why did I do it?" was: "It was forbidden", goes on to make even more explicit the curious logical connexion between God's creative activity and man's willful rebelliousness. He says, "And wherein did I, even corruptedly and pervertedly, imitate my Lord? Did I wish, if only by artifice, to act contrary to Thy law . . . so that . . . I might imitate an imperfect liberty by doing with impunity things which I was not allowed to do, in obscured likeness of Thy omnipotency?"¹ And then, elsewhere, doubtless having the same case in mind says: "For souls in their very sins strive after nothing else but some kind of likeness to God, in a proud, preposterous, and, so to speak, servile liberty".²

A similar tie between God the radical creator and man the radical destroyer is shown in Albert Camus's brilliant essay, *The Rebel*. He says of modern revolt: "Metaphysical rebellion is the means by which a man protests against his condition and against the whole of creation. It is metaphysical because it disputes the ends of man and of creation. . . . When the throne of God is overthrown, the rebel realizes that it is now his own responsibility to create the justice, order and unity that he sought in vain within his own condition and, in this way, to justify the fall of God"³—that is, to become God himself.

Again, the American poet, E. E. Cummings, seems to suggest that man as a radical destroyer of the world is the most apt antithesis to and hence the best source of analogies for God as creator *ex nihilo* when he writes:

¹ *Confessions*, Bk. II, ch. vi.

² *On the Trinity*, Bk. XI, ch. v.

³ *The Rebel*, trans. by Anthony Bower (Hamish Hamilton, London, 1953), pp. 29-31.

"when god decided to invent
everything he took one
breath bigger than a circustent
and everything began

"when man determined to destroy
himself he picked the was
of shall and finding only why
smashed it into because"¹

Finally, in Dostoyevski's novel, *The Devils*, we are confronted by Kirilov who, believing that God does not exist, nevertheless so conceives of the God who does not exist and who therefore must be replaced, that only an act of suicide by him is a genuine earnest that he himself may be thought to have become this God. He says : " Full freedom will come only when it makes no difference whether to live or not to live . . . a new man will come, happy and proud. To whom it won't matter whether he lives or not. . . . Everyone who desires supreme freedom must dare to kill himself."² In other words, the indifferent contemplation of suicide seems for Kirilov to exhibit a posture in relation to oneself and the world that is in some way or other like that which God has been thought to have to the world which is his creature.

Preliminary to analyzing further the significance of these striking parallels, it is necessary to consider what can be meant by the concept 'world', for it seems to me to be a very ambiguous one, and its ambiguity is the source of much confusion concerning what is meant by God as the creator of the world. Being perforce brief, this will be vague.

Doubtless there are many more uses of the concept 'world', but let us consider here only three.

'World' can be used, as Kant seems to have thought, as an idea of the Transcendental Reason, and as such functions as a regulative principle for a *cosmologia rationalis*. The concept in this use has no content—in Kant's sense—but nevertheless provides us with guide-lines in the pursuit of a goal which, though never to be achieved, nonetheless governs the progress of scientific understanding. This use of the concept is largely irrelevant to my present inquiry.

The world may also be thought of as that which can be exhaustively catalogued by a, practically speaking, infinite number

¹Poems : 1923-1954 (Harcourt, Brace and Co., N.Y., 1955), p. 404.

²Penguin Classics, trans. by David Magarshack (London, 1953), pp. 125-126.

of straightforward subject-predicate sentences in a language system which we will, in order to educe the distinction essential to my purposes, imagine as having no use of first personal pronouns, singular. In fact, of course, if we eliminate first personal pronouns, singular, it is difficult to imagine what pronouns like 'we' and 'you' could do in the language (where they are steadfastly held to be unanalyzable into demonstratives like 'this', 'that', 'these', and 'those'); and therefore we may eliminate them as well. Now we have a language with only the demonstratives, and the third personal pronouns. But surely, a language which does not use the first personal pronoun would have to reduce even the third personal to 'it', i.e. 'he', 'she', and 'they' could only mean what could be catalogued in reports of behaviour (actually we should have to say "events") or dispositions to behaviour. We now have a language in terms of which nothing can be said about persons.

In the language thus truncated, the world will be the sum of synthetic propositions that could conceivably be shown to be true or false. It would, in other words, be the world that could be known to us, and which is thought of in our scientifically dominated culture, as the world of common sense, and of all of the sciences themselves. Therefore it will be the world as it is "known by science". It will be objective, that is, it will be what can be catalogued exhaustively in a language having no personal pronouns. It will be in practice, a third person world, remembering that the only pronoun in the third person which remains is 'it'. It will be, in other words, the public world as it must be imagined to be apart from anyone actually experiencing it. It is the world as we would all agree it *must be*; all epistemological relativism aside, in the language of our model, it cannot be described as being experienced by anyone in particular.¹ It is a world in which there are no persons, because 'I' and 'my' cannot be used in the language that describes it. Therefore, it cannot be the world *of* or *for* anyone. No doubt a description of the world in this way would be a *tour de force*, and would involve a language which is very awkward, when compared with our ordinary ways of speaking about the common sense world. Nevertheless, I think it an imaginable one, and is in fact the ideal goal of all objective scientific knowledge, albeit the concepts that may be meaningfully used varies among sciences.

In contrast with this, and as our third use of the concept

¹ Given the purposes of my model, it is an irrelevant criticism to observe that modern physics has had to argue that an ideal observer or at least an observation point always has to be posited.

'world', let us imagine what we could speak of in a language in which there *are* first personal pronouns. What differences would immediately appear?

First the world would be *of* and *for* someone. What *I* would mean by the world would be *my* world—though, committed as I believe we usually are to using 'world' in the second of our three senses, this may be obscured. What is meant by 'world' in this sense would include all of those features which could be catalogued in "third person" language as in sense two. But by adding the first personal pronoun all of this would be radically transformed by the additional characteristic of the world being mine; not just the world as I experience it from a particular point of view in a third person way like the third person way in which you, from a different particular point of view, also experience it (in other words, the first personal pronoun does not merely introduce the possibility of epistemological relativism); but mine in the sense that I have a relation to the body, its behaviour and the environment of its actions (which is the world *for me*), which is part of what I mean by 'I', that can never be identical with the relation which *you* have, and that cannot be expressed in the language lacking the first personal pronoun singular. That this statement would seem to be analytic does not weaken the force of the distinction.

Perhaps this can be illustrated in the following way. In the terms set forth above, the expression 'I will die' when used by me cannot be exhaustively analyzed into a purely third person reading of 'This body will undergo a radical change, including ceasing to behave in certain ways, etc.' References by me to the body and its behaviour which is part at least of what I mean by 'I' can never be made to be logically equivalent to references by me to bodies and their behaviour which are not. 'I will die' can never mean for me just the same thing as 'There is a body in the world (in the second sense) which one day will cease to behave as it now does.' What is being asserted is not just about an object in the world (in the second sense) in the way that 'This body (as a component of the world in the second sense) will die' is about an object in the world. My body and its behaviour is not in the world *for me* in the same way that your body (to avoid the possessive pronoun, we'll call it "Smith") and its behaviour is in the world *for me*. For me to describe my death as the end of certain kinds of behaviour in the world is not *for me* the description of an occurrence *in the world* at all like an account by me of Smith's death as an occurrence *in the world*. For myself, I am not *in* the world as Smith is in the world.

Now, taking as our paradigm Kirilov's suggestion that the act of suicide will be the earnest of his having achieved Godhead because it will exhibit a characteristic in himself which he conceives to be essential to Godlikeness, how may the posture of a man to himself and to his world as he contemplates suicide be likened in certain respects to the posture of God when he is thought to be the "maker of heaven and earth"?—and it must be remembered that the parallel we are here drawing is between God as radical creator and man, as in some sense, a radical destroyer.

If we take seriously our distinction between the use of the concept 'world' in the second sense above, where what is meant in the nature of the case cannot be something that is *of* or *for* someone in particular; and its use in sense three, where it is always *my* world; then I think it is quite meaningful, using 'world' in sense three, to say that my suicide is an act of destroying *my* world. If, that is, we keep in mind how 'world' is functioning here, we may say that when I take my own life, I destroy the world! As destroyer of the world, in this sense, I stand to the world in my act of radical destruction, as God seems to be thought to stand in his act of radical creation. The posture I assume toward myself and the world (in sense three) as radical disposer is logically different from that I assume as disposer of this, that, or some other characteristic or feature of myself or the world. I am not destroying something or other *in* the world. I am destroying the world as a whole. I may be thought, in other words, speaking metaphorically, to take up a relation to myself and the world *as a whole*, to stand "outside" myself (in our ordinary uses of 'myself'); and this bears some analogy to what the Christian seems to be believing about God's relation to the world when he declares him to be "maker of heaven and earth".

Or, to put the case in a slightly different way, in the act of suicide I am, with reference to what I name with the personal pronoun 'I', bringing something radically to an end. Just as Hamlet's question "To be or not to be . . ." is logically not like "To be or not to be a doctor, lawyer or merchant chief . . .", so contemplating the ending of my life is logically not like ending a job or a marriage. It is an end of *all* possibilities for something, namely, for what I name with the personal pronoun 'I', and not just the ending of certain possibilities such as this or that. We can say "After his divorce he was remarried", or ". . . he was sadder but wiser". To go with the expression 'After he died . . .' there are no expressions logically like 'he remarried' or 'was sadder but wiser'.

I want to say then that though the act of suicide may not be thought of as destroying the world insofar as it is taken as an object for thought in the third person, nevertheless, the world as *my* world, in which part of what I mean when I use the pronoun 'I' of myself is to be found, which is the environment of the acts of the body that is part of what I mean by 'I', and is accordingly the world in our third sense, is destroyed. And the posture which I have in relation to the world, thus construed, is analogous to that which God is thought to have to the world (in either of our first two senses) as its radical creator.¹ There are three equally important features of this analogy to be emphasized. First, as destroyer of the world (in sense three), I have a view of myself as what may be called a radical agent. In the act of suicide, I perform an act which makes nothing out of something. My act is the reverse of God's who makes something out of nothing. A world which can be imagined, in terms of the present analysis, to have an end can equally be thought to have a beginning. When I imagine the end of the world by imagining a state of affairs in which there is no longer the world *for me*, I am thinking of a situation logically no more nor less queer than when I think of the world as having been created.

Secondly, in thinking of suicide as an act of destroying the world, I am thinking of the world as coming to an end, that is, as being finite. There is in this the greatest possible contrast with the way that we, quite properly, think of the world when using the concept 'world' in sense two. The world in the third person, the subject of our purely scientific curiosity, is in practice, and rightly so, open-ended, infinite, and therefore not a possible object of experience, as Kant seemed to imply in his refusal to make the concept either an empirical one or one of the understanding. Accordingly we can derive no analogy for God's relation to the world which is his creature, from our relation to the world in sense two, for nothing ever radically begins or ends in *this* world.

Thirdly, it is having this kind of relation to the world (in sense three) which I have as a radical agent, that constitutes, for the Christian, my being in the image of God.

II

It still remains for me to show that there are certain logical pressures within what we ordinarily say and think about

¹ There is a dangerous pitfall in the analogy here, for we may be tempted to infer from it, at this stage, that God and his world are identical. But this would be an invalid inference for, as I shall show, I and *my* world are not identical either.

ourselves as persons which are not less "queer" than, because logically analogous to, saying of God that he is "maker of heaven and earth". To do this I will undertake to analyze the expression 'I was born'.

Frequently we find ourselves answering questions such as "When were you born?" And we do not take these questions to be odd in any way. We answer by giving a date, such as "In April of 1919" or "Shortly after the First World War" or if the context is appropriate, we may say "In the year of the great earthquake", etc., or we may be asked "Where were you born" and answer "In Kaifeng, China" or "In the Prebyterian Hospital." If we are to take these questions as in some way mystifying we would probably be taken to be resorting to, perhaps suspicious, evasion, or to be mere trouble-makers. For the questions obviously presuppose possible answers in terms of straightforwardly datable and locatable events, which occur in the objective world which is spread out in time and space; and which, in practice, extends infinitely backward and forward from the event expressed by 'I was born'. As such, the event may be thought to have all the complexity that any of the events in this world has: one may, for example, consider it from the standpoint of historical chronicle, from the standpoint of biology and genetics, or from the standpoint of obstetrics. And saying that 'I was born' is true will certainly entail that certain historical, biological and genetic, or obstetrical propositions will be true. One may even wish to go so far as to hold that the proposition 'I was born' can be exhaustively analyzed into all the propositions of the same logical sort as those above, the truth of which would be entailed by the truth of 'I was born'.

We take 'I was born' in this way most of the time, and quite rightly so, as is evinced in our willingness to answer the question "When were you born?" by straightforwardly offering a date, and not precipitating any philosophical quarrels about it.

But if we take this legitimate because, in most cases, quite adequate interpretation to be the paradigmatic or only one, we are left with some serious puzzles. For this puts me in the curious position of *celebrating* a chronicle of events, or biological and genetic or obstetrical facts, and the like, when I celebrate my birthday! To honour and observe duly with solemn rites only certain obstetrical facts seems a very odd form of behaviour. And in fact I do not think any of us is doing this when we celebrate our birthdays, however impossible it may be to conceive of there being something to celebrate unless there *were* or *had been* some obstetrical facts. If we analyze 'I was born'

into the sum of true propositions about obstetrical and other facts, and the like, which are entailed by the truth of the proposition 'I was born', then there seems to be nothing left of the sort that as persons we celebrate—nothing, indeed, in which we could take other than a purely obstetrical interest.

Now, why is this so? I believe we can say that it is because concealed in the language in which we are asked "When were you born?" and in the answer "In April of 1919" is a subtle commitment to the objectivist language which possesses no personal pronouns or else does not take them seriously; and therefore a birthday can never be described as *my* birthday. Or to put it differently, if we take the question "When were you born?" to be like "When did you come into the world?", we answer the question in such a way as to predispose us to take the meaning of "into the *world*" in the second of the two senses above. It is obvious that in this view, my "coming into the world" is not a radical event. When do we start counting "being in the world"? At conception? At the moment I leave my mother's womb? Or do we start with the gleam in my father's eye? Birthdays are celebrated only by persons, for a birthday is not what we understand it to be unless it is *of* and *for* someone, unless there is only one person who uses the pronoun 'mine' of it. Only what can be *of* and *for* someone can be celebrated; and nothing in a world described merely in a language lacking personal pronouns can be so described. The fact that we celebrate birthdays therefore suggests that 'I was born' cannot be exhaustively analyzed into reports upon obstetrical events, etc., in our language having no personal pronouns.

If, however, 'I was born' thus analyzed tells some of the story of my coming into the world because the proposition 'I was born' entails that there will be certain obstetrical and other facts, what is left out? The answer is of course that the world into which I am described as having come is the world in sense two above. And there is not *for me* any world in sense three until I use 'I' and 'mine'. With this act, the world in sense three comes into being, the world which is *my* world. And it is an absolutely novel act, for only I can use 'I' and 'mine' *for* and *of me* and *my* world. The absolute discontinuity between there being no world in sense three for me and there suddenly being such a world because someone uses 'I' and 'mine' of it is the same as the logical discontinuity between language two having no personal pronouns and language three which does have them. And this is paralleled in the ontological discontinuity between there being nothing and there being something which is involved

in the doctrine of *creatio ex nihilo*. In the act of suicide I make nothing out of something, the reverse of God's act of creation. In using the first personal pronouns singular, I make something out of nothing. It is from this that our analogy for God as "maker of heaven and earth" must come. To speak of God as creator is not the same as saying 'I was born', but it is in certain respects logically like this.

III

Let us then ask in conclusion how some of the apparent conflicts between science and religion—with particular reference to the notion of creation—can be shown to be the result of a confusion concerning the logical status of their respective claims.

Earlier on, I suggested that the implicit ideal of all objective scientific inquiry is a catalogue of everything there is in a language from which we might imagine all the personal pronouns to have been dropped out, for such a catalogue yields a world which is as it is independent of its being known from any particular point of view or by anyone in particular. I also suggested that within limits imposed by the programme itself this is a perfectly legitimate enterprise. Let me now anticipate what is to follow and suggest that the putative conflict between science and religion—between Fred Hoyle and Genesis—respecting creation is the result of failing to notice the subtle commitment in this programme to the concept of 'world' as I have defined it as sense two above; and further because of a failure to notice that many of the things we say and think quite ordinarily about ourselves as persons rather operates with the concept in sense three. Finally, if our analogy for creation is to be drawn from the kind of discourse in which I say of myself 'I was born', where this expression is analyzed as unassimilable to a language having no personal pronouns and therefore must be understood as at once saying more than can be said in such a language while entailing all of the sorts of things that can, a compounding of the confusion is always possible. For this means that what is named by 'I' and 'world' in our language possessing personal pronouns is not entirely unrelated to the body and its environment that is described in the language which lacks them, because the concepts 'I' and 'world' (in sense three) could not be used were not the concepts 'body' and 'world' (in sense two) already in use—while the converse is not the case. Our language having personal pronouns says more than one lacking them, but not more in the sense of adding further information of the same logical sort

as already known through the pronounless language. Adding personal pronouns changes *the whole picture*, but it is an already familiar picture that is *transformed*, seen in a different light. This means that, in speaking by means of our analogy, of the world as having been created, we run the risk of misconstruing the relation of religious claims about creation of the world to scientific claims about the world in either of two ways: (1) supposing there to be absolutely no connexion between them;¹ (2) supposing the connexion to be of the sort obtaining between two propositions within one of the language systems in our model. To say that the world is created, using our analogy, is not to report an additional fact about it like saying that there are material objects in it. Saying of the world that it is created stands to the fact of its being extended in a way analogous to saying of a body that it is a person stands to its having three dimensions.

Now, perhaps the difficulties can be elucidated in the following way. Let my body—in so far as it is extended, an organism, and capable of what might be called directed activity—be the kind of being about which the physicist, biologist and psychologist speak. Within their conceptual schemes the notion of creation as the emergence of absolute novelty does not appropriately operate. Why? Because of everything that may be conceived as being reported in the concepts of the aforementioned disciplines we can imagine asking “What was the cause of *that*? ” (where ‘cause’ functions in a straightforward explanatory way), and we would expect to be given an answer by reference to some antecedents on a common logical footing with the events or behaviour reported by these disciplines, using a covering law which embodies the concepts of these disciplines. Each of them, in other words, takes any given event or piece of behaviour reported by means of their concepts to be continuous with other events or behaviour of logically the same order prior to the one reported and so on *ad infinitum*. While obviously the physicist, biologist and psychologist operate with differing concepts appropriate to their own modes of explanation; and even though one may wish to hold that for this reason there is logical discontinuity of a sort among these modes of explanation, since, e.g. no complex of physical facts *qua* physical logically entails any biological fact; there is an analogy among these modes in the respect that within the limits of their explanatory interests any fact, event or piece of behaviour is preceded by a theoretically infinite series of facts, events or pieces of behaviour of the same logical order. And their

¹ Karl Barth and Rudolph Bultmann, in different ways, seem to me to come very close to suggesting this view.

explanatory interests, far from requiring a notion of a radical discontinuation of these series, would in fact be frustrated by it.

The implicit ideal of these accounts of the world is a catalogue in a language in which there are no personal pronouns.

When, however, we add the personal pronouns everything is changed. However much the use of this new and enriched language may imply the appropriateness, within the specified limits, of the truncated one, we are now speaking of the world as *my* world and *yours*. A body and its environment may be thought of as becoming *my* body in *my* world—which is to use ‘world’ in the third of our senses—when someone, namely you or I, can use of them the expression ‘*my* body in *my* world’. Since nothing in the truncated language logically implies the new components of the richer one, we may say that *my* body in *my* world is, as *mine*, radically discontinuous with *this* body, *a* body, *the* body, etc., in the world (taken in the *second* sense). And it is this fact that makes it possible to speak of *me* and *my* world as having come into being *out of* nothing, i.e. as having been created.

To have shown the source of the analogies by means of which the Christian speaks of God as “maker of heaven and earth” is not the same as showing that these analogies ought to be used.

I have attempted here only to argue that the doctrine of creation *ex nihilo* is a logically queer notion; but that it is not as remote as is supposed from many things we say and think about ourselves in quite ordinary ways, and that an analysis of the kind of posture a man may be thought to assume to himself and his world in contemplating suicide and of the expression ‘I was born’ display this fact. Persuading a man that he ought to think of the world as having been created is not unlike persuading a man who speaks a language having no personal pronouns that there are persons.

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III.—THE PARADOX OF THE LIAR

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In this paper I succeed in showing, I think, that the paradox of the liar is a fraud : that it arises as the result of a mistake, and that but for the mistake there would be no paradox ; and that it illustrates nothing of the nature of language, but illustrates only people's proneness to oversights. The whole matter would be almost a joke, if it were not for the fact that a good many intelligent people have devoted a good deal of time to thinking about the problem.

In the first part of the paper I state, and try to refute, two well-known attempted resolutions of the paradox, and I then state my own view of the logical status of the sentence which is supposed to give rise to the paradox. In the second part, I give my own resolution of the paradox, using a method that has been found useful before in philosophy in dealing with sceptical and paradoxical arguments, and which consists not so much in attempting to answer the arguments, as in attacking them, and trying to show that they are fallacious and therefore need no answer.

(1) The paradox of the liar is usually presented in the following form. On an otherwise bare blackboard is written "The sentence written on this blackboard is false", and the question is whether what is written on the blackboard is true or false. The paradoxical argument runs, that if the sentence on the blackboard is false, then since this is what it proclaims itself to be, it is true ; and that if it is true, then since this denies what it asserts itself to be, it is false. (It is, of course, statements, and not sentences, that are properly describable as being either true or false, but the arguments which follow will be somewhat complicated expressed even in terms of sentences, where one has the advantage of inverted commas : expressed in terms of statements, they would be practically unintelligible. Where I talk of a given sentence being either true or false, I can be taken as referring to the statement which that sentence would normally be used to make.) Before giving my own views about the paradox, I want to discuss two very well-known attempts at resolving it, which seem to me to be unsatisfactory.

The first attempted resolution consists in maintaining that a sentence cannot ever refer to itself, but only to another sentence, and that since there is only one sentence written on the black-

board, this must either refer to itself or to nothing at all, and must therefore be nonsensical—or if not nonsensical, at least neither true nor false. But is it in fact the case that a sentence cannot refer to itself? On the radio a year or two ago there was a talk given by a man who was famed for the naturalness of his broadcasting style, and during the talk he said something to this effect: "Many people have asked me how it is that I can speak so unhesitatingly for fifteen minutes every week without a script: the fact is that I can't: every word that I say to you, even the words I am speaking now, are carefully thought out, written down, and read into the microphone." Now this is surely a case of a sentence referring to itself, or in other words, of a statement about the sentence which is being used to make that statement, just as in the case of the paradox of the liar. To take a slightly different kind of example: if the sentence on the blackboard were "The sentence written on this blackboard is in English", this would be a perfectly sensible and perfectly true sentence; and if it were "The sentence written on this blackboard is in French", this would be a completely sensible and completely false sentence: and each sentence would quite definitely refer to itself.

It is in fact rather rash to make such a wide general statement as that no sentence can ever refer to itself. It may be that in the case of a particular sentence, such as that in the paradox of the liar, the sentence, though appearing to refer to itself, in fact does not (or even cannot) do so; but anyone who wishes to maintain this must demonstrate it by a detailed analysis of the particular sentence.

There is another version of this same theory,¹ which maintains that while a statement may say something about the *sentence* which is being used to make it, a statement cannot ever say anything about *itself*. And this also seems to me to be wrong. Suppose the broadcaster I've mentioned had said: "I always consider very carefully, and discuss with friends, everything that I say to you, including what I am saying now." This would be a case of a statement which is saying something about itself: the speaker is asserting that he has carefully considered and discussed with others both *what* he is saying and how he is saying it. Some of his friends may, for example, have suggested he should let his listeners believe that he always thought up what he said as he went along, while other friends may have argued that he should tell his listeners that this was not so. The speaker is

¹ I am indebted to Professor Ryle for the suggestion that I should discuss this version of the theory.

not saying whether or not this was in fact the way the discussion went, but he is saying that there was a discussion. Similarly, if someone during the course of a B.B.C. broadcast says, "This programme is coming to you from our West of England studios", this statement, being itself part of the programme, tells us something about itself: the speaker is not making any general assertion about the sentence he is using, that would be true (or false) of that sentence wherever and by whoever it was used (for example, that the sentence is grammatical), but is telling us something about his particular use of that sentence at that particular time and place; in other words, something about the statement he is making. Again, suppose a club secretary says to the members, "Our President agrees to my informing you of his intention to resign at the end of the year". The secretary is in this case saying something about what he is saying at the time: he is not only announcing the president's intention of resigning, but is saying also that the president agrees to this announcement being made. There is perhaps a *limit* to what a statement can say about itself. In the last example, the club secretary is not saying whether or not the president agrees to the members being told that he agrees to their being told about his intention to resign (although one would in fact be very surprised to hear that he did not agree). But even if there is a limit, it is not, I think, possible to say in general where the limit must lie.

The second attempted resolution of the paradox runs as follows. To say that something that someone has said is true, is merely to agree with, or to re-assert, what he has said: it therefore follows the "The sentence written on this blackboard is false" is equivalent to just "The sentence not written on this blackboard", which is not a sentence at all, and the question of its truth or falsity therefore does not arise. But is it the case that to say that something or other is true, is merely to agree with, or to re-assert it? If this were so, it would be pointless to have to certify in a formal declaration, for example, in an application for employment or for an insurance policy, that "the statements made above are true to the best of my knowledge and belief", since this would amount merely to certifying that you agreed with what you had said, or that you were willing to re-assert it: what you are required to certify is that what you have asserted to be the case, is in fact the case, and that nothing you have said, however willing you may be to say it again, is a lie.

A further point is that we do not normally "agree" with someone on matters of knowledge, but only on matters of opinion,

whether it is opinion about facts, or about questions of value or taste, or about the truth of statements. If someone makes a statement which we know quite well to be true, for example, "The Conservatives have been in power for over seven years now", we might well say "That's true", but we would never say "I agree". And even when we can, as on matters of opinion, say appropriately either "That's true" or "I agree", the two answers do not always amount to the same thing. Suppose you hear a statement made, and the person you're with comments "That's false"; if you then say "I agree", he will take you to be agreeing with him that the statement you both heard was false, but if you say instead "That's true", he will take you to be disagreeing with him, and maintaining that the statement you both heard was true. When we say that a given assertion is true, we are doing more than just asserting the same thing again, or agreeing with the person who made it.

Another illustration of this thesis is the fact that one can make not only true, but also false, statements, and not only in order to deceive, but for many quite respectable reasons: for example, in cracking a joke, or as a way of starting off a discussion, or in order to make someone happy or to avoid giving them offence. It sometimes happens that a person tells you something which strikes you as ludicrous, and which you take as being in fact intended as a joke, but then from the expression on his face it occurs to you that perhaps it was not intended as a joke, and you ask "Is that true?": if he says that it is, you may well no longer regard the matter as ludicrous, but think of it instead as being sad, or even tragic. This would be hard to explain if the question "Is that true?" were merely a request to the person to re-assert what he has asserted. You know that he has already said it once, even if you did think he asserted it, not as the truth, but as a joke. It also happens sometimes that a very sick patient, whose doctor tells him that he will soon be well again, asks the doctor "Are you telling me the truth?"; but by this question the patient is certainly not asking the doctor whether he really asserted what he did assert. In short, would there ever be any point in asking a person whether what he had said was true, if this were just another way of asking him to say it again?

To describe a statement as being true (or false) is to say something about that statement. It is to say, firstly, that you are regarding that statement as one which is designed to convey information. And it is not, of course, as has already been argued, always appropriate to regard a statement in this way: a father who tells his child a bed-time story regards it as an irrelevant

question if the child asks whether the story is true. Secondly, by describing a statement as being true, you are also indicating, or actually stating, what the information is that you suppose is intended to be conveyed : you are indicating it if you say merely "That's true", or stating it in so many words if you use a sentence of the form "It's true that so-and-so is the case". Thirdly, you are asserting that what the statement you describe as being true says is the case, is in fact the case ; or in other words, that the attempt to convey information, which you assume on the part of the person who made the statement, was successful, since the facts are as stated. And fourthly, by using the expressions "is true" and "is false", you are claiming not only that the statement you refer to is one which is designed to convey information, but that what you yourself say is intended to convey information, and is not intended, for example, as insincere flattery or as a joke : you are re-asserting what has already been asserted, but in a particular way, namely in a way designed to convey information about the world : if one wanted merely to indicate the other person's intention to convey certain information, without oneself claiming to convey it also, one could say, not "What X said is true", but "What X said was seriously meant". So, in short, to describe another person's statement as being true, in addition to merely re-asserting what he has asserted, is also to imply an intention on his part to convey certain specified information, and to state an intention on one's own part to convey the same information, and to assert that the other person's attempt to convey information was successful.

Now so far as the paradox of the liar is concerned, it is certainly important to establish, as I have tried to do, that to describe a statement as being true or false is to say something about that statement. But to determine the logical status of the paradoxical sentence one needs to do more than this : it is necessary to establish that what is properly describable as being true or false is always a sentence (or statement), or a group of words that could stand alone as a sentence (or could be used to make a statement), *which does not itself contain the words "true" or "false"* ; and this is, I think, proved, or at least strongly supported, by the following argument. If someone said, "It's false that the Conservatives are out of favour at present", and you replied "It's true", you would be taken as asserting that the Conservatives *are* out of favour at present. Now why should this be so ? If the sentence you were referring to were "It's false that the Conservatives are out of favour at present",

to say that this was true would be to say that the Conservatives are *not* out of favour at present, since you'd be saying in effect "It's true that it's false that the Conservatives are out of favour at present". In fact, the sentence you are actually talking about is just "The Conservatives are out of favour at present", a sentence not containing the word "false". I do not want to maintain that the word "true" or the word "false" can occur only once between a capital letter and a full-stop. They can occur as many times as one likes: one can say, for example, "It's true that it's false that it's false that it's true that the Conservatives are out of favour at present". What I am saying is that *all* the "true"'s and "false"'s are descriptions of a certain group of words, that could stand alone as a sentence, and which does not contain the expressions "is true" or "is false"; and that they are all descriptions of the *same* group of words.

Consider how someone would work out, in the case of a sentence containing a lot of "true"'s and "false"'s such as the one just mentioned, what it amounted to, so that he could decide whether or not he agreed with it. He would take the last "true" or "false" first ("true" in this example) and say "That means it's true that the Conservatives are out of favour". He would then add on the penultimate "true" or "false" ("false" in this example) and say "It's false that it's true: that means it's false that the Conservatives are out of favour". Then he would add on the preceding "true" or "false" (the latter in this case) and say "It's false that it's false: that means it's true that the Conservatives are out of favour". Then finally he would add on the first "true" or "false" ("true" in this instance) and say "It's true that it's true: that means it's true that the Conservatives are out of favour". And so he would conclude, "What all this amounts to is saying that it's true that the Conservatives are out of favour at present. I agree, it is true", or "No, it is false". (But the fact that all the "true"'s and "false"'s in a given sentence can be, and, to make the sentence intelligible, often must be, reduced to one, does not mean that they can all be reduced to none at all, as the second of the two theories discussed earlier maintains.) In short, we can use more than one "true" or "false" in a particular sentence, but they will all refer to another sentence (or group of words that could stand alone as a sentence) which does not itself contain the words "true" or "false". It must be a whole sentence, or a group of words that could stand alone as a whole sentence, because a person cannot be capable of truth or guilty of falsehood unless he asserts that something or other is the case,

and he cannot do this in anything less than a sentence. (Of course he can shout "Fire!", but what his hearers will understand him to mean by this will be what could be expressed by the complete sentence "The house is on fire" or the complete sentence "Fire your rifles now"). And it must be a sentence which does not itself contain the expressions "is true" or "is false", because a sentence containing these expressions, as has already been explained, does more than assert merely that something is or is not the case: if this second condition did not hold, the words "true" or "false" would mean different things when used to describe a sentence which did not itself contain either of these words, and when used to describe a sentence which did contain one or other of them.

The implication of all this for the paradox of the liar is that the sentence "The sentence written on this blackboard is false", written on an otherwise bare blackboard, is nonsensical. This is so, either because what is being described as false is a group of words, namely "The sentence written on this blackboard", which could not stand alone as a sentence; or because what is described as false is a sentence itself containing the word "false", which does not in its turn describe another group of words, capable of standing alone as a sentence, and not containing the word "false". So whether the sentence on the blackboard is interpreted as referring to the whole of itself, or to only part of itself, it is still nonsensical, due to an infringement of the rules governing the use of the words "true" and "false". It is as nonsensical as is the sentence "The word 'house' is false".

So far I have not been particularly original, since both the other two theories I discussed also concluded that the sentence written on the blackboard is nonsensical, although I would claim that I have produced better arguments for this conclusion: in contradiction of the first theory I argue that the sentence can perfectly well be regarded as referring to itself, but that in doing so it infringes the rules governing the use of the word "false"; and in contradiction of the second, I argue that the word "false" does have a definite and distinct function, but that in the case of the sentence on the blackboard this function is not being fulfilled. But even if all I have said so far is correct, this still leaves the most interesting problem unresolved: how and why does a nonsensical sentence give rise to a paradox? I shall argue that the paradox is a fake, and arises because the formulation of the paradoxical argument leads one to believe that what is being referred to is a different group of words from the one that is actually being referred to.

(2) One's suspicions about the paradox of the liar are aroused when one notices that if the same argument is applied to a perfectly sensible sentence, it also leads to a paradox. Take "The sentence 'Socrates is still alive' is false". The argument will still run, that if this is true, then since this denies what it says it is, it is false; and that if it is false, then since this agrees with what it says it is, it is true. How does the trick work? The basis of the trick is the fact that there are two ways of considering the truth or falsity of a sentence like "'Socrates is still alive' is false". We can either ask ourselves the questions "Is it in fact false that Socrates is still alive?" and "Is it true that Socrates is still alive?" (and these are the questions we would normally ask). Or we can ask the more unusual questions "Is it true that it's false that Socrates is still alive?" and "Is it false that it's false that Socrates is still alive?" Now the first of each of these pairs of questions are equivalent to one another, and the second of each pair are equivalent to one another, since it just is a convention in English the "it's true that it's false" means "it's false", and "it's false that it's false" means "it's true". It does not matter, therefore, which formulation we choose for our questions, but we must decide on one, and remember which one we have chosen: we must know what we are referring to. In the argument "If it is true, then since this denies what it says it is, it is false; and if it is false, then since this agrees with what it says it is, it is true", what do all the "it"'s refer to? Do they refer to "Socrates is still alive" or to "The sentence 'Socrates is still alive' is false"? If it were clear which was being referred to, no paradox would arise: a paradox only arises because the argument switches from one to the other. But this needs some more detailed explanation.

I think it would be agreed that the only way one can decide whether an ordinary statement is or is not true is by appeal to the facts of experience; and that the only conclusion that can be drawn from *supposing* or *assuming* that a statement is true, is that it is true (or something equivalent to this), and from supposing or assuming that it is false, that it is false. Consider to begin with the group of words "Socrates is still alive". The first part of the paradoxical argument runs: if (that is, "assuming") it is false, then since this is what it is declared to be, it is true. Now the first and second "it" in the argument refer to "Socrates is still alive", but the third "it" does not: this refers to "'Socrates is still alive' is false". This must be so because if one assumes the falsehood of the sentence "Socrates is still alive", and notices that this coincides with what the

sentence is described as being, the correct conclusion is that the *description* of the sentence, but not the sentence itself, is true : the conclusion is that it's true that it's false that Socrates is still alive, or in other words, that it's false that Socrates is still alive, and this is just what was assumed at the outset. One starts off by assuming that "Socrates is still alive" is false, and one concludes, not surprisingly, that it is false : a paradox only arises if one imagines that the last "it" in the argument refers to the same thing as do the first two. Similarly with the second part of the argument—the assumption that "Socrates is still alive" is true. Since this contradicts what the sentence is described as being, the description is therefore false, and so the conclusion must be that it's false that it's false that Socrates is still alive, or in other words, that it's true that Socrates is still alive, which again is what was assumed at the beginning.

Consider now the paradoxical argument as applied to the complete sentence "'Socrates is still alive' is false". It is, certainly, quite legitimate and meaningful, as has already been argued, to assume during the course of the argument that this sentence as a whole is false, and then that it is true. But the point to notice is that the assumption of its falsity—the assumption that it's false that it's false that Socrates is still alive, or in other words, that it's true that Socrates is still alive—is not equivalent to the complete sentence, but contradicts it. And so the first part of the paradoxical argument will run : if it's false that it's false (that is, if it's true) that Socrates is still alive, then since this denies what "Socrates is still alive" is originally asserted to be, the original assertion is therefore false, and so it is false that it's false that Socrates is still alive : the conclusion is identical with the assumption, as it should be. And similarly, the assumption of the truth of the complete sentence "'Socrates is still alive' is false"—the assumption that it's true that it's false that Socrates is still alive, or in other words, that it's false that Socrates is still alive—does not contradict the complete sentence, but is equivalent to it. The second part of the paradoxical argument will therefore run : if it's true that it's false (that is, if it's false) that Socrates is still alive, then since this agrees with what "Socrates is still alive" is originally asserted to be, the original assertion is therefore itself true, which means that it's true that it's false that Socrates is still alive : again, the conclusion is identical with the assumption.

To put the whole matter in quite general terms, we have a certain group of words, which we can call "*p*", which is originally asserted to be false. We start off by assuming that *p* (which

is asserted to be false) is in fact false, and we notice that this agrees with the assertion that it is false, and so we infer that the original assertion of the falsehood of p is itself true; in other words, we infer that p is false, which is what we started off by assuming. We then assume that p (which is described as being false) is in fact true, and noticing that this contradicts the description of p as being false, we infer that it is false to describe p as being false, or in other words, that p itself is true, which is what we assumed. I shall now try to show that this same line of argument, although in a slightly more complicated form, also holds for the paradox of the liar proper.

In the case of "The sentence written on this blackboard is false", written on an otherwise bare blackboard, since this is a nonsensical sentence, the two conventions already mentioned—that "it's true that it's false" means "it's false", and "it's false that it's false" means "it's true"—no longer holds good. Or rather, it would be more accurate to say that one has to decide whether or not they are to hold good. But I hope to show that, whichever decision one makes, no paradox arises.

The situation is, however, complicated by a further fact. We all realize that only a sentence, or a group of words that could stand alone as a sentence, can properly be described as being either true or false, and so since the group of words "The sentence written on this blackboard" could not stand alone as a sentence, there is an almost irresistible tendency to interpret the sentence on the blackboard as reading "'The sentence written on this blackboard is false' is false". But again I hope to show that whether one takes "The sentence written on this blackboard is false" as it stands, which is tantamount to assuming that the group of words "The sentence written on this blackboard" could stand alone as a sentence, or whether one interprets it as reading "'The sentence written on this blackboard is false' is false", or indeed whether one interprets it in any other way, no paradoxical conclusions emerge. The important point is that, during the course of the paradoxical argument, reference is made to some group of words which "is asserted to be" or "is described as being" false, and it is absolutely essential to decide just what group of words this is.

Assume to begin with that the two conventions I've mentioned do hold good, and that the sentence on the blackboard is taken as it stands. In that case, the first part of the paradoxical argument is: if the phrase "The sentence written on this blackboard" is false (that is, if one postulates "The sentence written on this blackboard is false"), then since this agrees with

what the phrase is described as being, the description is therefore a true one : in other words, the conclusion is " 'The sentence written on this blackboard is false ' is true ", which just means " 'The sentence written on this blackboard is false ' ", which is what was postulated. Similarly with the second part of the paradoxical argument : if the phrase " 'The sentence written on this blackboard ' " is true (that is, if one postulates " 'The sentence written on this blackboard is true ' ") then since this denies what the phrase is described as being, the description is therefore a false one : in other words, the conclusion is " 'The sentence written on this blackboard is false ' is false ", which just means " 'The sentence written on this blackboard is true ' ", which is what was postulated.

Now suppose that the two conventions—that " it's true that it's false " means " it's false ", and " it's false that it's false " means " it's true "—do not hold good, but that the sentence on the blackboard is still taken as it stands. In that case, from postulating " 'The sentence written on this blackboard is true ' ", and noticing that this contradicts what was originally asserted, the conclusion " 'The sentence written on this blackboard is false ' is false " certainly does follow, and this is no longer equivalent to just " 'The sentence written on this blackboard is true ' ". But the conclusion is still not a paradoxical one, because what is asserted to be false in the original sentence, and to be true in the postulate, is the group of words " 'The sentence written on this blackboard ' ", whereas what is asserted to be false in the conclusion is " 'The sentence written on this blackboard is false ' ". The conclusion is incommensurate with the postulate and with the original sentence, and cannot be made commensurate with them by combining the two " false " 's in the conclusion : it cannot therefore be said to contradict the postulate. Similarly with the other part of the paradoxical argument : from postulating " 'The sentence written on this blackboard is false ' ", and noticing that this is the same as what was originally asserted, the conclusion " 'The sentence written on this blackboard is false ' is true " does follow, but it is not a paradoxical conclusion because again what is asserted to be false in the original sentence and in the postulate is a different group of words from that which is asserted to be true in the conclusion. The conclusion does not contradict the postulate because it is incommensurate with it, and it cannot be made commensurate with it by combining the false and the true of the conclusion. It may sound queer to say that the conclusion of a correct inference cannot be compared with that from which it is inferred, but then queer things are

bound to happen when one assumes that some of the normal conventions of language no longer hold good.

In short, if the two conventions are assumed not to hold good, there is no paradox because the conclusions are not comparable with the postulates, and therefore do not contradict them; and if the conventions *are* assumed to hold good, there is no paradox because the conclusions not only do not contradict the postulates, but are identical with them.

At this point it might be objected that it is just too absurd to argue that in the sentence "The sentence written on this blackboard is false", what is asserted to be false is the group of words "The sentence written on this blackboard"; and that it is also absurd to suggest that by assuming this sentence to be true, one is postulating "The sentence written on this blackboard is true", and by assuming it to be false, one is postulating "The sentence written on this blackboard is false". It might be maintained that what is asserted to be false in the case of the paradox of the liar is the whole sentence "The sentence written on this blackboard is false", and it might be pointed out, as I have done in the first part of this paper, that if someone says "It's false that so-and-so is the case", one can contradict him by saying, not only "It's true (that so-and-so is the case)", but also "It's false that it's false (that so-and-so is the case)"; and so, it might be argued, to postulate that the paradoxical sentence is true, is to postulate "'The sentence written on this blackboard is false' is true", and to postulate that it is false, is to postulate "'The sentence written on this blackboard is false' is false". Now this is a perfectly reasonable line of argument, but even granted that it is correct, this still does not mean that any paradoxical conclusions will be produced.

If what is written on the blackboard is taken as it stands, and if the normal conventions governing the combination of "true"'s and "false"'s are assumed not to hold, the postulates "'The sentence written on this blackboard is false' is true" and "'The sentence written on this blackboard is false' is false" will be incommensurate with what is written on the blackboard, and so no conclusions whatsoever will be possible. If, on the other hand, the sentence on the blackboard is taken as it stands, but one assumes that the normal conventions *do* hold, the postulate "'The sentence written on this blackboard is false' is true", since it will be equivalent to "'The sentence written on this blackboard is false'", will *agree* with what is written on the blackboard, and therefore the conclusion will be the identical and non-paradoxical one, "'The sentence written on this blackboard is

false' is true"; and similarly the postulate "'The sentence written on this blackboard is false' is false" since it will be equivalent to "'The sentence written on this blackboard is true'", will *deny* what is asserted on the blackboard, and so the conclusion will again be identical with the postulate.

If in the paradox of the liar "'The sentence written on this blackboard is false' is true" is to be the postulate of *truth*, which is to *deny* what is asserted on the blackboard, and "'The sentence written on this blackboard is false' is false" is to be the postulate of *falsehood*, which is to *agree* with what is written on the blackboard, this means that what is written on the blackboard must be interpreted as reading "'The sentence written on this blackboard is false' is false". Only in this case will the first of these two suggested postulates deny what is asserted on the blackboard, and the second agree with it. (If the sentence on the blackboard is taken as it stands, the first of these suggested postulates will *agree* with what is asserted on the blackboard, and the second will *deny* it; and this case has just been discussed.)

Suppose then that what is written on the blackboard is interpreted as reading, "'The sentence written on this blackboard is false' is false". One can here, I think, dismiss from consideration at the outset the two extreme assumptions as to whether or not the normal conventions governing the combination of "true"'s and "false"'s hold good—the assumption that they hold for every "true" and "false" in the argument, and the assumption that they hold for none of them. If the conventions were assumed to hold good absolutely, this would produce the absurd result that the sentence on the blackboard (interpreted as reading "'The sentence written on this blackboard is false' is false") would be equivalent to "'The sentence written on this blackboard is true". And if the conventions were assumed not to hold good at all, this would mean that the two conclusions in the paradoxical argument would be incommensurate with their postulates, since they would contain an additional "true" or "false", and could not be made commensurate with them through the combination of "true"'s with "false"'s. The only realistic assumption is, I think, one intermediate between the two extremes: that in "'The sentence written on this blackboard is false' is false", the "is false" following "blackboard" is *not* combinable with any "true"'s or "false"'s that appear after it, but that the second "is false" is combinable with any "true"'s or "false"'s that may appear after it. I shall therefore argue on this assumption. It does not much matter, however, what assumptions you make because no paradox

arises provided you remember just what assumptions you *have* made, and remember how you have interpreted the sentence on the blackboard, and bear in mind all the time just what group of words it is that you are referring to.

So interpreting what is written on the blackboard as reading “‘The sentence written on this blackboard is false’ is false”, and assuming that the normal conventions governing the combination of “true”’s and “false”’s hold for the second “is false”, although not for the “is false” after “blackboard”, the following argument will show that no paradoxical conclusions emerge. What we have is a case of a group of words, namely “The sentence written on this blackboard is false”, which is itself described as being false. Now assuming that this group of words is in fact false, then since this is what it is described as being, it is true that it is false, which means that it is false, which is what was assumed. Assuming that this group of words is *true*, then since this denies what it is described as being, it is false that it’s false, which means that it’s true, which again is what was assumed. The conclusions, far from contradicting the postulates, are identical with them, as one might expect if the conclusions are correctly drawn.

To end with, I should like to state the main part of the argument of this paper in quite general terms. If one has a group of words, “p”, which is described as being false, then the assumption that p is true, since this contradicts what p is asserted to be, implies, not that p itself is false, but that the description of p as being false is false, which means that p itself is true; and this is identical with the assumption. And the assumption that p is false, since this agrees with what p is asserted to be, implies, not that p itself is true, but that the assertion of p to be false is true, which means that p itself is *false*, which again is identical with the assumption. And this argument holds good whether or not p is a group of words that could stand alone as a sentence, and whether or not p itself contains the expressions “is false” or “is true”.

IV.—DE SOMNIIS

BY FRANK B. EBERSOLE

ON the pages where modern philosophy is sired, I am asked to suppose that I am dreaming. I am asked to suppose this as an exercise which will help me see that it is possible for everything to appear in every way as I now think it is, but really be different—there might conceivably be nothing at all except me dreaming. So many important things have come to depend on one's ability to do this exercise. It shows the distinction between "hard" and "soft" data. It shows the relevance of "logical possibility" to the analysis of knowing. It enables one to glimpse the experiences of seeing, believing, remembering, hoping, intending, inferring, etc., unencumbered with non-essential elements. Four centuries of philosophy testify that the exercise is basic and important. Also we have it on the authority of Descartes that this sort of inquiry should be engaged in once in his life by everyone who has the faintest love for truth. These promises are compelling, and this challenge more than I can resist. Since apparently my meditations have been loose and superficial on this point, I resolve to set myself more seriously than ever before to master this very essential bit of philosophy.

(A)

Do I understand what I am to do? I am to suppose or imagine that I am now dreaming—not merely to exercise my powers of imagination, but in order to see that I might be deceived about everything (or almost everything) as I have been deceived about everything while dreaming.

How do dreams deceive me? Well, when am I deceived? I have often been deceived by *people*, but no one is involved now except myself. I have also been deceived by *appearances*. "I am amazed", I say. "Your house gives the appearance of being very small. I would have sworn you couldn't have more than three rooms this size. As I was coming up the walk, the appearance was deceiving. It looked so small. I thought it was much smaller than it is." This is the way I am deceived by appearances. Instead, suppose I dreamt that I visited your house, and while walking up to it I thought, "My, this house is made of gingerbread with white frosting. It's very small." How was I deceived then? I was deceived when I thought your

house was small ; I found it to be much larger than it appeared to be. What I thought was shown wrong. I revise my belief. In my dream, too, I thought your house was small. Do I need to revise my dream-belief ?

What sort of question is this ? I think of two circumstances where questions like this might be asked. First, if I were telling my dreams and said, "I thought it was small . . .", a listener might well expect me to add, "but I went in and found I was wrong". If I did not say what I found upon going in, he might ask, "Did you have to revise your belief?"—requesting me to tell some detail of my dream which he thinks I left out. Secondly, suppose I were telling my dream to some friends, and someone joined my audience as I was midway through my story. He listened while I said, "I thought your house was very small". Now he asks, "Weren't you wrong ? Didn't you have to revise your belief ?" I will tell him he misunderstands. He arrived late and missed my prefatory remarks : I was telling a dream. I dreamt I thought the house very small. Both these are questions which others may ask of me when I have described a dream, but what question can I ask myself ? I know the dream I had, and whether in the dream I did or did not have to revise my beliefs. If I dreamt that I walked on into the house, and said, "My, how spacious, but no difference ; I thought it was small and it is", I should not be surprised. Things go like that in dreams. I will not now, while thinking back on the dream, say, "That was wrong of me. I should have revised my belief." While telling the dream I might say, "I knew that was wrong of me. I ought to have changed my belief, yet I didn't", but I would say this only if it were telling part of the dream. When I ask, "Should I revise the belief I had in the dream ?" I am not asking myself whether I have forgotten some detail of the dream.

I must try to construe my question as like the second. It is almost as if I were a man who had walked in on a dream story in the middle—only the dream story is my own. Almost as if I had forgotten I was telling of a dream, and now I ask, "Did I make a mistake ? Ought I to revise my belief ?" Well, I will say the question is like that, and so I will answer that it is a misunderstanding. I dreamt I thought it was small. I made a mistake the day I walked to your house, and thought from the appearance it was small. But when I dreamt it was small I made no mistake. In the waking-life case there was a clash between my belief and what I discovered when I looked inside. There is no clash between what I dreamt and the facts, because what I dreamt was

just a dream. I feel uneasy about answering in this way, because there is no such question I can ask myself about being wrong.

I seem to be floundering over the very first step in the exercise. If there is no question, I cannot go on. If I accept my answer, where did I go wrong? I find no incompatibility between dreaming that I believed your house is small, and the fact that it is quite large. Oh yes, but I found out I was dreaming only after I was awake. While I was asleep, I thought your house was small. I am back at the same misunderstanding. While I was asleep I did not think your house was small. Sleepers are not thinking about things at all. When I was asleep I was dreaming. I did not think it; I dreamt I thought it. But of course when I was asleep, I did not know I was dreaming. All the same, I was dreaming. People who do not know they are watching the tricks of a magician may have mistaken beliefs about what is going on. But people who do not know they are dreaming when they are dreaming, are not making any mistakes. When I am deceived, I may refuse to recognize it. I may even argue for the truth of what I thought I saw. In such a situation I argue for a falsehood. That may be so, but how many falsehoods can one maintain when he is sound asleep? The question is based on a misunderstanding—if I can imagine myself having such a misunderstanding. So I cannot be deceived while dreaming about your house. Perhaps this is because, while dreaming, I can not cognitively get at your house. But I can get at my own dream. Suppose I had a dream of myself in a very queer and frightening room. There was a sickening purple light everywhere. I said to myself, "I mustn't be afraid, because this is a dream". Then an old friend appeared and said, "No, you are wide awake and your fears are right, for we are all about to be hanged". Then I knew it was not a dream; I knew I was awake. At this point, I woke up, bewildered to find myself not in a strange, purplish room but in my own bedroom.

Here I feel I was deceived in a dream. But was I? I said, "I know I am awake", and I was asleep. No. I did not say it. I dreamt I said it. What I dreamt I said is not the important thing; I believed I was awake. No. That does not change the matter: I dreamt I believed I was awake. Even when I dream about whether I dream or not, I am dreaming the question and the answer; I am not asking the question or giving an answer. I cannot see how one can make a mistake in a dream at all. The idea of it is more and more ridiculous. Suppose someone said, "I missed three questions on the exam; two while writing in the

classroom, and then I dreamt I missed another". How many questions *did* he get wrong? In order to carry out the cartesian exercise, it seems I am required to act like a late listener to my own dream story, one who does not know that what is described is a dream. I have to try to underwrite his misunderstandings in my own name. But then I have to reject them. I am not making much progress, certainly. Where have I gone wrong? For I certainly am sometimes deceived by a dream. Oh yes, I know how that happens. I hurry to get my appointment book out of a desk drawer. It is not there, and then I remember: I did not put it there; I dreamt I put it there. How stupid, but that *was* a very vivid dream. Could not I be deceived like this right now—because I am dreaming? I do not see how from the example, because in it, I was not deceived *while* dreaming. I was deceived because I was led to look in my drawer for my appointment book. While I was dreaming I was lying on the couch. While dreaming I did not think my appointment book was there; I dreamt I put it there. Even if I had dreamt I wanted it, and in my dream I remembered putting it there, I would not have gone to the drawer looking. I would have stayed right there on the couch, sleeping.

Am I not deceived only because I do not walk in my sleep? I am not certain what sleep walkers do, except walk in their sleep. Suppose, anyway, that they dream while they are walking. I am watching a sleepwalker who wakes with a shock, finding himself with his hand in a desk drawer. Suppose he says sheepishly, "I dreamt I was looking for my appointment book". I say to him, "It isn't in your desk drawer". He might reply, "Oh, I know it isn't, but I just dreamt it was". In the proper philosophical tone, I point out, "Well! Then I caught you being deceived." He might say, "Er—yes, I was certainly mixed-up". The trouble is, he could just as well say, "Oh no, I didn't want the damn book. I certainly didn't need it—don't need it now. I wasn't even looking for it; I was dreaming I was looking for it." Or again, he might say, "I guess I thought my book was there". Where any of these would do, no *one* of them is *the* right description. Perhaps his inability to tell what he thought, and whether he was deceived is due to his confused state of mind. I, the observer, am not in this befuddled state. I have been coolly watching and listening. What would I say? Did he believe his book was in the drawer? Was he looking for it? Was he wrong in his belief? I certainly know what happened: he walked as if drugged, he shuffled through the drawer, then he awoke puzzled and astonished. This is something *like* what one does when he wants something,

has a wrong idea about where it is, and is surprised not to find it there. However, when a person is wide awake and searching for something, I can ask him what he is doing, what he wants, and his answer will usually settle any questions I have about what he is after and what he thought. The sleepwalker who wakes with his hand in a drawer cannot give clear answers to my questions. When a person who is awake and sane looks confidently for something—right where he remembers putting it—and finds it is not there, he does not wake up at that point, terminating abruptly all familiar developments. What the sleepwalker did was something *like* acting on a false assumption all right, but also something very unlike. Does it belong under "false opinion" or does it not? Well, how like does it have to be to belong? There is no criterion, no way of deciding, and it is easy to see why there is none. Usually it would make no difference which way we put it: no misconceptions would arise. When one can say any of several different things and accomplish the same purpose, they come to the same thing, perhaps "they mean the same thing", i.e. we would say they mean the same thing in the circumstances in which they would be said. In circumstances in which they are not said, then they are just not said. If there were any reason for it, I could make a decision. I could decide to say that sleepwalkers who have dreams that correspond in the proper ways to their waking behaviour have false beliefs. But my decision to say they have false beliefs would not settle anything about whether they have false beliefs or not.

Of course this decision-making is bewitching business. Once one is committed to it, he is required to make more and more decisions, each a new challenge to one's ingenuity, because we want them made in an orderly and consistent way. What of the man who wakes up in the nightmarish dream that a spider as big as a great Dane is sitting on his chest? His heart thumps, he sweats and screams. Does he believe there is a spider on his chest? For how long? While he is screaming? His heart races for five minutes after awaking. Shall we decide he believes for five minutes? It may colour his behaviour for an hour: he may get up and be fearful of going back to bed. He may return to bed but be afraid of the dark room and keep a light on. Shall we decide that he believes for an hour or two? Until he turns off the light? Regardless of how these questions may be decided, for those who sit and sympathize with him in his fear, it makes no difference whether they say he believes in a chest-riding spider or not. For those who want to settle some point of theory, the *truth* is he does not either believe or not believe.

I understand well enough how one might be deceived *by* a dream, but I am supposed to imagine how one might be deceived *in* a dream. Since my hypothetical sleepwalker does not provide a case, I am beginning to despair of finding one. Of course there is the great range of hypnagogic states, of experiences of half-sleep, of times when one is not quite but almost dreaming, and so forth. I might wake slowly to an overcast and gloomy day, and lie suspended between sleep and waking. I might feel as if I were floating on a magic carpet. I wonder where I am. I feel alarmed. I feel I am moving, but yet I see—now—I am lying on my bed.

The trouble with such cases is that everything is "mid-way" and "as-if" and "as-it-were". One can say, "I believed I was floating, then I discovered I was not". And one can also say, "It was as if I were floating, then I discovered I was not". And one can also say, "It was as if I were floating; it was almost as if I believed it, not quite a dream, not quite awake. Then it seemed as if I made a discovery—much as if I found out I was wrong." Here again the background and the cues would make all this clear.

I cannot now see how any such examples can give me a case which I would unhesitatingly describe as "being deceived in a dream". I am almost convinced that it is impossible to be deceived in a dream. I almost want to say that even if I were dreaming now, I cannot see how I would be deceived. I would not be operating on any false beliefs, because it is not clear that I would be operating on any beliefs at all. I would be dreaming that I was operating on certain beliefs. Yet this is a queer thing for me to want to say, because when I am dreaming, I do not know what I am doing. I cannot say anything about what it would be like to be dreaming now, because while dreaming I am not in any position to say what it is like. Anyway this would be to despair of success in the cartesian exercise.

(B)

I must try to imagine myself in a position to say what it is like while I am dreaming. Maybe it is not necessary first to understand about being deceived in dreams. If I could come to see the possibility that I might be dreaming now, then perhaps I could see how I might be deceived. Although it sounds as if I must crawl into a dream while still awake—a most unlikely occurrence—I will keep trying. I will try to imagine that I am now dreaming. Is it not easy enough to imagine almost anything I want to

imagine ? Yes, when the occasion is right. It is the proper occasion that I cannot imagine. Someone just says, "Everything you know—everything you have—is a dream. Even now you are dreaming." Can I imagine that ? I begin to feel a weariness come over me. I think that one by one the things I love are taken away. The old times are gone, all the brave and fine things. Once the springs came, moist ; and the nights were warm ; so deep the solitude ; tears, too, and now no child's tears. All this is gone, and I am waking ever from a dream ; yet the dream goes on. Here am I, then, now dreaming—feeling sorry for myself. Is this a philosophical exercise ? My philosophy has dissolved into sentiment, or worse.

At the very end of *Through the Looking-Glass* Alice says to her kitten, ". . . let's consider who it was that dreamed it all. This is a serious question, my dear . . . You see, Kitty, it *must* have been either me or the Red King. He was part of my dream, of course—but then I was part of his dream, too". Finally, I, the reader am asked, "Which do *you* think it was ?" Well, I think it was I dreaming and they were all in my dream, and I am dreaming still ; for I can turn back the pages, and lo ! the adventures go over again, but I know how they are going to turn out as I sometimes do in dreams. Now my philosophy has turned into nonsense. I do not want nonsense or sentimentality ; I do not want soft or silly, I want hard imagination.

The trouble is, I cannot think of the right occasion ; I cannot think of anyone asking, "Can't you imagine you are now dreaming ?" when he does not want to moralize or sentimentalize, or just have a little verbal fun. I want a situation which will call on my logical and analytical powers. For the time being, I guess I will just have to forget about the situation. A philosophical directive is given to me, "Imagine you are now dreaming !" I will just repeat to myself, "Be hardheaded, do *hard* imagining !" All right, I will try some logico-philosophical steps. If I am now dreaming I must now be asleep. I must imagine I am asleep. How can I do that ? To imagine I am asleep, I must imagine, and to imagine I must be awake. That is no good ; that is like saying "I can't imagine what it will be like in the middle of the next century, because by then I will be dead, and dead men don't imagine". Or suppose that an astronomer is lecturing on the solar system. He says, "Let's imagine we are on the sun". I will not say, "I can't do that because I couldn't survive on the sun to imagine". Of course I will not say that. He only wants me to think how the planets and stars would look from the sun, if I could be there.

But the cartesian philosopher does not ask me to imagine how things would be at a different place, or in a different time. He does not want me to imagine myself at a different place or time; but to imagine myself, right here and now in a different state—dreaming. In that state I could not be imagining, I would be dreaming. That is all I have shown. All right, I imagine myself in that state; I just cannot put too much of myself in, for I must remain in this state, imagining. I imagine I am now asleep, and dreaming. What more is required? Now I must imagine I am dreaming *all this*. I see all this, my desk, my pen, etc. *No*, I do *not* see it; I dream I see it. Only when I am dreaming I do not know that I am dreaming. Of course not, but I do not see things either. This line of thought merely takes me back to all my difficulties about deception, and the queerness of talking about dreams in the present tense.

When asked astronomically to imagine myself on the sun, it was not important that I imagine myself there in some super-insulated, air-conditioned cell. The question was to consider what the planets would look like from thereabouts. Perhaps the philosophical question is what would all this which I now see look like in a dream. But I neither look nor see in dreams; I dream I look and see. There is my mistake again: I have brought dreaming back in. That is as irrelevant as my insulated shell on the sun. I am just to imagine what it would look like. From where? When? Here, now. Silly. It looks like this; only in a dream. No, I must forget the dream part; but how can I? In order to imagine myself philosophically dreaming, I must make sure not to imagine myself dreaming.

Surely I can do better. I have assumed that saying, "Do philosophy; be rational; hard work, now!" would enable me to escape from imaginatively constructing the circumstances in which one might say, "I am dreaming". At the great risk of not knowing what I am talking about at all, I have tried to get inside a dream. Still it is impossible to escape from where I am. I cannot say "dream" without being reminded of dreams. I cannot talk about dreams, without talking at least indirectly about dreams. I seem to be held down from the required flight of imagination by the dead weight of my words. There seems to be something to say, but language prevents me from saying it. So I must go back, and begin again. I must imagine the circumstances where one would say, "I am now dreaming", and this context must not be such as will lead to sentimentalizing or nonsense, or anything like that. I need to imagine someone talking in his sleep, saying, "I am dreaming". Then I need

to imagine myself in his place. That looks fearfully hard to do. Maybe I can get inside a dream more easily if I take it in two jumps. First I will just get access to the dream, then later I will get inside. How to get access to a dream? That is easy. I will just imagine someone, say J, describing a dream. He tells what happened in his dream. Suppose he says that he dreamt there was a street fight and that he stood at a window watching as if history depended upon his remembering the details of that scene. He describes all this, in the past tense of course, as one does when he recounts a dream. Now I will imagine myself doing something like that while awake; I simply describe a street fight which I saw some time ago from my window. If I can now imagine that my description and his description are the same sort of thing, I will have access to a dream while awake. They are both "describings", but beyond that I can not see how I am going to construe them as the same thing at all. I am telling what people did on the street. Insurance companies might have an interest in my account. They could have no interest in J's story, in spite of the fact that he dreamt that his watching was important. My listeners might want to check up on my story, for my prejudices may distort my report. What I say can easily be checked by interviewing people who were on the scene—or better by watching a movie made at the time of the fight. My description can be doubted for all sorts of reasons, checked in all sorts of ways. But what of J's description of his dream—or for that matter any description of a dream? When it is honestly told, given in considerable detail and with confidence, it makes no sense to doubt anything. Nothing anyone could do or say would ever count as "checking up on the description of a dream". Describing a dream and describing what has been observed while awake are clearly very different. Then what I am doing can by no stretch of the imagination be what J is doing.

So I cannot make it inside a dream from that jumping-off place after all. All right, I will try to make it in one big jump. I imagine someone (call him N) lying on a couch. His eyes are closed; he breathes slowly, the rhythm of sound sleep. He says, "I am dreaming". What would I make of this? First off, I might think he was awake: he's making a philosophical joke. No. This interpretation is ruled out because I must imagine him asleep. He is asleep, and he says, "I am now dreaming". What do I say? Well, he is talking in his sleep all right. How do we take what people say in their sleep? It all depends: it depends on what they say, what we know about them, the purpose for our

listening to what they say, etc., etc. If a sleeptalker insults me, I am sure I cannot collect libel from him, even if I have a tape recording of his words. Suppose that a man while talking in his sleep describes an invention. I realize its importance and have it patented. He is not an inventor, does not know anything about it, does not remember talking in his sleep. Have I stolen the idea from him? I think I would feel uneasy about it. Suppose a man in deep sleep says, "I promise to meet you for lunch tomorrow". If he does not show up for lunch, he has not gone back on a promise. He made no promise. Suppose he said, "You will find a hundred dollar bill in your middle desk drawer". I look in my desk drawer and find no money. Was his assertion false? No. He can no more assert than he can promise. While talking in sleep, one cannot make claims or assertions. If N, my hypothetical sleep-talker, is not asserting that he is dreaming, how can this case help me?

Maybe these questions will be clearer if I fill in the example with more details. While sleeping, N says, "I am dreaming", and then he goes on like this : "And I am dreaming of a large, very large Siamese cat, larger than a lion, and he leaps at me now out of the bushes. I am running . . . etc." Suppose, also, that when he awakes he relates a dream. He says, "I dreamt of a gigantic Siamese cat which leapt at me out of the bushes . . .", and he goes on like that telling the details of a dream which perfectly corresponds to what he said he was dreaming while asleep. Suppose, moreover, that N's performance is regular : whenever he talks this way in his sleep, he has the corresponding dream which he relates when he wakes. When he does not talk in his sleep, he claims to have no dream. All this is what N can do. Now I imagine him asleep on his couch talking. He says, "I see a street fight from my window . . .", and he goes on telling about it.

I want to imagine myself in his place. I imagine a news reporter calls me on the phone, asks me to describe what is going on below my window. I tell him as accurately as I can the details of a street fight. While I am describing what I see, I ask myself, "May I not be doing the same thing as N there, talking in his sleep? Could I now be dreaming?"

When I am giving a running commentary of what takes place on the street below me, I am describing what I see. Is that what N is doing? He does not see anything. His eyes are closed and he is sound asleep. That is good ; that brings out the philosophical point, because I must imagine that I do not see anything either. I dream that I see this fight in the street. Then I must dream that I describe it. Does N dream that he describes his

dream? A trumpery detail. We will say that he does. When he tells his dream he says, "And I was standing by, describing all this". He dreams he describes what he dreams; still, the question is, does he describe his dream? He talks in his sleep clearly enough and what he says has this amazing point by point correspondence with what later he describes as a dream.

In one important matter, the case of N sleep-talking is more like reporting on events in the present tense than J's describing his dream is like my describing past events. They are more alike in this matter of doubt and subsequent checking. Suppose discrepancies occur between N's sleep-talk and his later description of a dream. Surely, then, the later, normal account is definitive. This is something like correcting what was first said. Only it is just *something* like it, and not *very* much, at that. It would not really be correcting what he had said, or finding he had made a mistake, because while sound asleep he had not made any assertions or put forth any claims. The question remains, was he "describing" his dream while he slept? I do not see that it would make any difference what I say to this. I have already noticed how very different describing a dream is from describing a street scene. They are made no more alike than they are by being counted two cases of describing. Likewise N's sleep-talk and my present-tense reporting of a street fight are either the same or different, quite without regard to what I would call or decide to call N's peculiar kind of sleep-talking. Which are they, same or different? Am I sure what I am asking? What do I mean here by "same?" What is same or different depends on the circumstances. If X lights up his pipe, and it is suggested that I do the same thing, I will take out my pipe and light up. This would be doing exactly what was suggested, namely, doing the same thing as X. Suppose I am invited to a Cheyenne Indian Council. The chief is smoking a pipe and he suggests I do the same thing. Then I would not be doing what he suggested if I pulled out my pipe and lit up. I would have to wait until he passed me the peace pipe in order to do the same thing he was doing.

In the case of N's sleep-talking, and my describing a street fight, the circumstances do not seem to be important. I want to be able to imagine that I am now, while describing a street fight, really sleep-talking like N. I may be doing what N is doing; I may really be in N's place. For this I can say *everything* must be the same, *everything*—except, of course, N is dreaming.

I say "everything", but I cannot make it work out the way I want. As soon as I put myself in N's place, *almost everything*

seems different. What he is doing, talking in his sleep, and what I am doing, describing what goes on around me, are very different. The first and most obvious difference is that I am aware of what I am doing. N is not ; he is asleep. This is one of the reasons why I am making assertions and he is not. I can be held accountable for what I say ; he cannot. Next, and this strains any likeness almost to the breaking point : I know perfectly well what I am doing. I do not know at all what he is doing—beyond talking in his sleep in the strange way that I have imagined him to do. No, I should not say that: it is very misleading. Rather I *do* know exactly what he is doing, but I do not know what to say about it. The point is, I know all there is to know. In this respect what N is doing is like describing a dream, because when someone tells me his dream, I know all there is to be known about his dream and his dreaming it. I know well enough what N is doing, lying there, talking away. I know he will soon awake and tell me his dream. What I am doing is describing something. I do not know whether to say he is describing anything or not. Even if he is, he is describing what he dreams he sees, not what he sees. The logic of talking about dreams is just stubbornly different from that of talking about things and events.

Even so, are the differences enough or of the right kind to be sure that these two occurrences are not the same? I assumed that saying, "everything—*everything the same*" would make it plain whether N's sleep-talking and my describing were the same or different. I found differences and concluded that N and I were doing different things. I should not have done that: in general, the presence of differences does not prove "*everything is not the same*". Suppose I am taking a cowhand to dinner and he is uneasy about his manners. I tell him, "Just do everything the same as I". If he uses the right spoon in the right hand, and so forth, he carries out my suggestion. "Fine. You did everything exactly the same", I'll say. This does not mean that he coughed when I coughed, smiled when I smiled and dropped his napkin when I dropped mine. Perhaps with all the differences I noted between my describing the street fight and N's sleep-talk, they are still exactly the same. Perhaps they are, but how can I tell? And if they are exactly the same, is he doing what I am doing—describing?

A child may ask me to show him how to sweep floors. I hand him an old broomstick and say, "Watch, and do exactly what I do". Then I take the broom and sweep, and he does exactly what I do. I was sweeping. Does it follow that he was sweeping, too—because he did exactly what I did? How could he sweep

without a broom? But if I had given him a broom in the first place, then his doing exactly what I was doing would mean that he and I were both sweeping. It does not follow from some general rule that if N and I are doing exactly the same thing, we are both describing. Nor does it follow from some general rule that what N is doing and what I am doing are cases of doing exactly the same thing. These matters depend upon special circumstances. What must I do now? Give a description of the conditions under which my questions arise?

I can imagine an insurance investigator who has heard tape recordings of N's sleep-talk and my report. He says, "I don't see that it makes any difference which recording we use in court because the two of you seem to be doing the same thing". I would certainly reply, "No. We weren't doing the same thing at all. You don't seem to understand. I was watching the fight. N was asleep at the time." I can imagine a teacher of journalism who wants to give his class an exercise in analysing a present-tense description of a street-fight. He is about to concoct one himself when he runs across the two tape recordings. He says, "I won't need to make one up. We'll use one of these." No matter which. Both are doing the same thing.

I am not interested in any such conditions. I want the situation right here and now, in which I am asking this question : were N and I doing exactly the same thing? Could we both be describing dreams? I ought not to need a description of these conditions because I am in them. If there are any conditions which are complete and which I understand, these must be the ones. Right here they are—now. I can see why I was inclined to say N and I are not doing the same thing, because most of the situations which come quickly to mind where the question might be asked are such that the answer would be unequivocally, "No. They aren't doing the same thing at all". I feel inclined to say my situation is more like these which come easily to mind than it is like the others. But what I am inclined to say is not what I do say, and I ought not to have to discuss how like to others and different from others my situation is. Here is my situation and I want the answer. The answer is, "I can't tell what to say. The situation is not such that I can give an answer."

If I were standing in a classroom with chalk in hand before a blackboard, I would accuse myself of the "classroom and blackboard fallacy". This fallacy is the assumption that the special conditions which make it possible to answer a question can miraculously be produced by writing an interrogative sentence on a blackboard. I am very familiar with it by being

very guilty of it. I am now beginning to think there is a counterpart fallacy, the fallacy of philosophical meditation, which is subtler because it calls for no classroom, no blackboard, no chalk. I was guilty of it. I can also see that I am getting tired and hasty, because I am strongly inclined to think that Descartes somewhere along the line must have been guilty of it too.

I thought it would be possible for me to imagine myself dreaming if I thought of something one did while dreaming which was the same thing I sometimes did while awake. Then all I would need to do was imagine myself doing the same thing. I thought of N's peculiar kind of sleep-talking, and my reporting of a street fight. First I thought N and I were not doing the same thing at all—so that I could not imagine myself now dreaming by pretending I could be in N's place while everything remained the same. I discovered a mistake in that conclusion. Finally, I have come to the conclusion that I cannot even tell whether N and I were doing the same thing or not. I cannot even get the question asked in such a way that I can see how it could be answered. And I now suspect that something is wrong with the question and not with me. If I cannot even ask the question, then I cannot possibly imagine by such means that I am in N's position now. I simply am unable to imagine that I might now be dreaming. I cannot see how one could say, and mean, the philosophical statement, "I might now be dreaming".

I must go slowly. Before I become too presumptuous, I had better lay aside this exercise and continue when I am fresh and have recovered from my first frustrations. I know this is an arduous undertaking, and I must be suspicious of fatigue, like that "indolence which leads me back to my ordinary course of life".

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V.—AScriptive AND PRESCRIPTIVE RESPONSIBILITY

BY SAMUEL STOLJAR

1

PROFESSOR HART's distinction between description and ascription is certainly one widely known.¹ In preliminary outline, the distinction conveys that sentences about human action such as 'He did it' do not primarily describe physical acts, but rather admit, assign, claim or (in a word) ascribe responsibility, much in the way that we recognise rights or courts of law pronounce judgments. For example, to say 'Smith did it' is often not merely to confirm the facts of the case, but is to draw a conclusion on the basis of given facts. In short, the distinction can roughly, but adequately, be modelised as follows :

- (i) Smith did it—yes or no ;
- (ii) Smith did it—guilty or not,

where in (ii) ' guilty ' or ' not guilty ' can be non-legal expressions of a moral or conventional kind. Thus stated Hart's thesis becomes simple enough, if only because (as he put it) timeless conclusions are not entailed by statements of temporal fact. Thus stated, again, the descriptive-ascriptive distinction closely corresponds to that between fact-stating and rule-stating discourse, a distinction generally accepted by modern philosophers.

However, in arguing the details of this distinction, Hart pursued a novel and peculiar course. Ascriptive statements, he tried to show, have a distinctive feature which he called ' defeasibility ', indeed a defeasible feature which ascriptive share with legal concepts. These concepts are defeasible since they cannot be contained within an exhaustive, descriptive formula, because no such formula could encompass all the relevant conditions by which a person's responsibility for an action would be decided or ascribed. This notion of defeasibility became Hart's main theme. And this, unfortunately, led him into arguments which attempted too much and (paradoxically) did also too little. For one thing, over-emphasis on defeasibility made Hart underrate the logical force of statements concerned with intentional or deliberate acts. For another, defeasibility produced a much wider uncertainty,

¹ See H. L. A. Hart, *The Ascription of Responsibility and Rights*, in *Logic and Language* (1st series, 1952) 145. See also J. L. Mackie, "Responsibility and Language", *Austr. J. Philosophy*, iii (1955) 143.

namely, how the facts describing an action are, or can be, related to ascriptions or rules. For though Hart repeatedly said that ascriptive conclusions are 'supported' or 'justified' by the facts, he never explained the manner of this justification or support. But just this explanation Hart's own account demands, since without it an inconsistency arises between asserting, on the one hand, that ascriptions are defeasible (and defeasible precisely because uncontrollable in any purely descriptive formula) and saying, on the other hand, that an ascription must be supported or justified by the facts. Thus after stating that ascriptive statements do not describe facts, nor are inductive or deductive inferences from them, nor can therefore be either true or false, Hart adds that a decision ascribing responsibility can be either right or wrong (p. 155). But these assertions do not really match. For even if the relation between description and ascription is admittedly not one of entailment, still facts and rules are closely linked : our very ability to say whether a decision is right or wrong pre-supposes some definite connection with the facts the decision is about. Not that Hart was unmindful of the relationship between statements about facts and those about rules. However, in discussing this he confined himself mainly to 'proprietary' statements such as 'This is mine (yours, his)'. For example, a statement like 'This is my will' can be a blend of descriptive and ascriptive uses in which sometimes the one, sometimes the other predominates ; and the last sentence can mean (ascriptively) that 'all my money is (or will be) yours', as it can further mean (descriptively) that 'this is my last intention' or 'the piece of paper I've just signed'. But these examples do not help very much, and the reason is that ascriptions of rights are very different from ascriptions of responsibility. One illustration may suffice. 'This is yours' can be freely expanded into 'This arsenic (or this sugar) is yours', and referring to 'this' or arsenic or sugar will not affect the recognition of the right : 'this' or 'it' will be 'yours' or not, quite irrespective of the object involved. On the other hand, 'He puts arsenic (or sugar) in the tea' clearly makes for different ascriptions in either case : in the one case 'he' would be a poisoner, in the other perhaps a considerate host. Hence it becomes obvious that while ascribing responsibility is most intimately related to the facts of the case, the ascribing of a right is, relatively speaking, in much looser connection with them. Further, the recognition of a property or similar right pre-supposes an already existing system of (what may be called) distributional rules ; it is much like winning points in a game where the player scores goals, makes runs or check-mates,

Be this as it may, this paper is to deal with ascriptions of responsibility and will attempt two things : to explain the intricate relationship between responsibility-ascriptions and the facts upon which these ascriptions depend ; and to differentiate between two such ascriptions : one type which is expressed or expressible in prescriptive rules, and a second type of ascription which is defeasible, indeed defeasible somewhat in Hart's sense. Inevitably, I shall also be much concerned with what 'defeasibility' is not.

2

To begin with an example of the most unpretentious kind. Suppose someone asks 'Who turned on the light ?', and the answer is 'Smith did it'. What sort of further questions can you ask ? Regarding one sort of question 'How did Smith do it ?', we would have to explain the mechanics of switches, electric currents and so on. To a second kind of question 'Why did Smith do it ?', the answer could be 'Because he wanted to (read, play or just waste light)'. Yet, thirdly, 'Why did Smith do it ?' would have to get a rather different reply. We would say that Smith should or should not have done it, or that he turned the switch inadvertently, or that he was challenged or goaded into it, or that there was an emergency. The third answer clearly assumes the existence of some rule for or against the doing of the act, or at any rate assumes that we wish, or are about, to make a rule concerning it. We can see that what distinguishes the three questions is what we ask and answer about the same facts. More specifically, our first and second questions (those relating to method and motive respectively) asked for a description of how Smith did it and what sort of person Smith is ; only our third question required an ascriptive turn since it was not answerable save in terms of some rule of behaviour either laid down in advance or to be laid down after the event. Now suppose that, in fact, there is a rule saying 'Do not touch the switch, no matter how or what'. If Smith thereupon touches or turns the switch, he can surely no longer defeat responsibility, and this because the given rule contains *ex hypothesi* all the conditions under which responsibility is to be ascribed. Moreover, now to say 'Smith did it' also gives us a statement free from any vice of defeasibility. But if so, how can we (on Hart's analysis) sustain a separate logical type of ascription when its (alleged) chief characteristic, its defeasibility, has disappeared ? This would be enough to establish, first, that the descriptive-ascriptive distinction cannot be supported on the ground of

defeasibility, and that, secondly, to sustain the distinction we have to view it as expressing in idea something of the now more familiar distinction between facts and rules.

But Hart's notion of defeasibility raises wider issues still, especially since he directed much-needed attention to the similarities between ascriptive concepts and legal ones, in particular 'contract' or 'intention' in criminal law. Contract, Hart contended, is a defeasible concept because any contractual claim can be met and defeated by a large and heterogeneous number of 'pleas' such as (to name only a few) fraud, duress or immoral purpose. Similarly, an accusation of the type 'He did it intentionally (or voluntarily)' can be challenged and defeated by many defences like accident, mistake, provocation, compulsion and so on. The main thing about these defences, according to Hart, is that no one common formula will cover them all. One could give some sort of summary of them, but the summary would at best be a preliminary, not to say primitive, outline for the student, or a 'compendious reference' to one or a few or a limited range of defences. In brief, one cannot say (in the manner of some jurists) that contract must be based on 'true, full and free consent' or that a 'responsible' or 'guilty' action requires the presence of a mental element or 'intention', fully specifiable or specified.

Let me deal with contract first, as it was Hart's chief paradigm. Admitting that the defences to a contractual claim can be numerous and disparate, what follows from this? One very obvious corollary is that one cannot provide one simple or short definition or 'test' of contract, both in its positive (legally valid) and negative (legally invalid) sense. A concomitant corollary is that to enumerate the necessary and sufficient conditions of contract would be a vacuous effort, because no enumeration would resolve the heterogeneity of such requisite conditions as consent on the one hand and such available defences as, amongst many others, lapse of time on the other. The enumeration, however useful pedagogically, would have no greater logical significance than a rather amorphous 'etcetera'. Yet even if we grant all this, the above corollaries can prove misleading. In the first place, to say that 'contract' is a defeasible concept (because indefinable by way of necessary and sufficient conditions or by way of a manageably short formula) is, in an important respect, but to say that there are, have been and will continue to be many different legal rules for different circumstances. Thus, among many possible examples, a bargain that is 'fairly and freely' entered into is different from one induced by fraud or

compulsion. The difference is that the fraudulent and the non-fraudulent bargain are two different situations, though exactly how different has now to be gone into.

Take a simple yet basic illustration of A and B having met, parleyed and agreed by shaking hands to exchange property for money. As a recital of facts, it is clear enough what agreement here means. Thus they 'agreed' and 'shook hands' mean, that they 'intended' or 'consented to' their agreement. No search is needed for a mysterious mental process called 'agreeing' or 'consenting'; nor need we verify introspectively (as Professor Mackie suggested) the existence of something called 'consent'. What at present matters, and all that is now necessary to say, is that the parties' co-operation in shaking hands, coupled with their expression for a mutual exchange, must mean that A and B agreed and that to that extent and at that moment they intended or consented to the exchange. If two people sing a duet, you do not have to add that they agree, intend or consent to co-operate vocally; their agreement is manifest while singing; and only when they stop and quarrel, would you say that they no longer agree to sing together. But returning to the main point, suppose that after A and B have agreed and shaken hands, A discovers that B has lied to him about (*e.g.*) the value of the property A promised to take. Although a court of law would now 'avoid' the contract on the ground of fraud, this does not mean that there never was a contract in the sense that the parties never agreed or that their transaction can never constitute a 'contract'. All it means is that a court will not enforce this particular contract, given the additional element of B's misrepresentations, and may refuse enforcement either on the hypothesis that A would not have shaken hands had B not misled him, or because of a public policy according to which economic exchanges should remain free from deceit. However, the point is not whether or why a court denies enforcement (for we are assuming the existence of fraud as a defence); the important point now is that we cannot even begin to talk of a contract being avoidable for fraud (or, for that matter, any other defence like immoral purpose and so on), unless it is settled that there is a 'contract' in the first place. A neat illustration of this is the defence of compulsion or duress, where the promisor complains that he would never have promised had not the promisee forced him to. Obviously, there would be no purpose in physically forcing out a promise, if that promise were not believed to be enforceable. The same is true of every other defence a promisor may plead, since his defence would make no sense

except on the prior (legal) recognition of contract. Taken together, these defences may be both numerous and heterogeneous, but they are defences *to* a contract; so that unless we can indicate what a contract is, we cannot establish the reference back of the heterogeneous defences. Indeed, precisely because the defences are so different amongst themselves, that they have no common characteristic other than their relation to the primary concept. The defences, in short, *depend* on a primary concept which in this case is contract or agreement.

Consider, in the second place, Hart's notion of the 'cash-value' of a concept consisting both of the positive and the many negative conditions which reduce or discount it. But, as will be seen, to ask for *one* cash-value of 'contract' is not merely to ask for a stern, it is to ask an impossible price. For if legal defences qualify or reduce claims, there still is not one cash-value but just as many cash-values of contract as there are contractual claims with or without their defences. There are, to repeat a perhaps obvious point, many different situations, in some of which a claim may be recognised without a defence, whereas in other situations one or several defences apply. Contracts are enforced every day without there being any question of fraud, or duress, or lapse of time, or any other legally recognised plea; contracts are also lost every day because some old defence is pleaded or some new defence is pressed and succeeds. The total pattern or (conversely) the cash-value of this cannot be summarised in a single formula from which all applicable results could be deduced. But then this is not at all required. When you deal with one contractual situation, you do not need to know whether there is, or is not, a contract in situations different from it. What this is leading up to is simply this, that even admitting that 'contract' as such is a defeasible concept or that its cash-value is often reducible, the admission is quite immaterial. Because the answer to the question 'Is there a contract?' does not lie in the concept 'contract', but lies in the numerous distinguishable situations each with its particular rules: it is the rules which fill up the concept, the concept does not unpack the rules. Yet this is not to say that 'contract' becomes an empty label or 'short-hand' tag. For the concept also expresses a functional unity of manifold rules: a descriptive unity indicating the fact that contractual situations have to do with (broadly) consensual activities; a logical unity created by the link between the primary rules about contract and the dependent rules representing the various qualifications and exceptions; and an ascriptive unity arising from the generating principle that bargains should be kept.

With the emphasis thus away from cash-valued concepts, it is easily possible to state basic contractual rules without fear of their defeasibility. For example, we can instruct a person that to make a contract he ought to nod or shake hands or express word of agreement, or to put it in writing ; or you can tell him not to tell lies about the bargain, nor to contract for an immoral purpose, nor to use physical compulsion and so on. Words such as 'lies', 'immoral' and others certainly have 'open texture', but this is a flaw which most words have. Nor is the openness of texture at present misleading or troublesome ; for to instruct someone not to use fraud or duress suggests a sufficiently wide range of cases for the rule or instruction to apply. The exact limit of that range is not now of interest, because we are concerned with the logical nature of certain rules, not making rules for a marginal case : we must first inspect the main body of the texture, its kind or type, before we can look at the fraying seams.

3

As Hart was misled by the defeasibility of contract, he seriously under-rated the 'intentional' element in certain acts. As regards this, Hart was mainly out to show the inadequacy of certain traditional and modern views according to which (very briefly) the major characteristic of an act is the intending or desiring of its physical consequences, at any rate a more or less consciously failing to avoid them. These views are inadequate because they present human action in a purely descriptive light, as though when we say 'Smith did it' mere evidence of a certain intention makes him 'guilty' without more. This, however, confuses state of mind with the ascription of guilt, for while the former may be a matter of factual inquiry, the latter is entirely a matter of rules. And this not only because we may ascribe responsibility in an 'absolute' manner, *i.e.* irrespective of a guilty mind, but also because even grim intentions are not a reliable key to our ascription-rules : a soldier's grimly killing enemies is regarded as heroism, not as homicide.

Still, this does not exhaust the logic of statements concerning intentional acts. To see this we need to pursue Hart's argument that 'intention' is, much like 'contract', a defeasible concept. For the accusing sentence 'Smith hit her' can be challenged, and partly or totally defeated, by a vast number of different pleas such as accident, mistake, self-defence, provocation or insanity. But again, and as in the case of contract, the statement 'Smith hit her' and the statements of the various defences refer to different

sets of facts. To say that Smith hit her accidentally is not to say that Smith hit her because mad or provoked ; and these statements, furthermore, do not exclude the accusation that Smith just hit her without accident and without being mad or provoked. Whatever defence we take, be it one already existing or be it a new defence which the courts later introduce, it will always be possible to contrast between, on the one hand, the sentence 'Smith hit her' and, on the other hand, a similar sentence qualified by some additional facts and embodied in the old or in the new defence. Unless these defences do cover some new or qualifying facts, the whole force of the original statement 'He hit her' would be entirely destroyed. Indeed, our rules would become self-contradictory, since we would be both ascribing and denying responsibility on exactly the same facts. Consider this in another way. According to Hart, an accusation that Smith 'intended' the act is defeasible by proving (*e.g.*) inadvertence or accident. But by the same token the defence of inadvertence must be defeasible too, if only because the prosecution would have to defeat that defence. But defeasible in what way ? Not, obviously, by suggesting that Smith was mistaken or insane ; because this would merely add another defence and not defeat the original one. It becomes clear that the only way of defeating the defence of inadvertence is to show that the act was an intentional or deliberate one. And so with every similar defence connected with state of mind. Thus, by looking (as it were) on both sides of the line, we discover not only many statements of the type 'He did it' which are indeed defeasible by one or several exceptions, but also an indefeasible core of 'He did it' quite unaffected by any or whatever defence. Our logical ledger, in other words, shows in one column a uniform entry, *i.e.* 'Smith did it deliberately', entirely defenceless and positive, while the other column contains a motley collection of entries representing every possible defence.

4

The entries in our ledger deserve another look. Why is it that our positive column has only one entry ('He did it deliberately'), compared with the other column where the entries abound ? The principal reason is that intentional harm can form the subject of simple and specific prescriptions or commands. Thus the single sentence 'Do not deliberately injure anyone' has a vast reference, ranging not only from a humourless push to a brutal kill, but including all means of contact from fists, to poison, to guns. This aspect of deliberate harm becomes even

clearer when looking at such curious but possible sentences as (i) 'Do not negligently cause injury' or (ii) 'Do not accidentally injure anyone'. Sentence (ii) makes no sense, because of the inconsistency between what is implied by 'do not' and what is implied by 'accidental'. On the other hand, we do say 'Be careful' or 'Do not be careless' and so on. But such phrases, unless uttered within obvious context, have no general import at all. For if told 'Do not be careless', what do you or can you do? You cannot even begin not to be careless if you do not also know what not to be careless about. Unlike the rule 'Do not commit intentional harm' which contains lots of immediately applicable instances, a command like 'Do not do careless harm' contains no instances of that kind. The explanation is that statements concerning careless harm are of indefinite and almost unlimited scope. These can include quite unexpected injury (*e.g.* Smith gently hitting Brown not knowing that he has a weak heart and Brown dies), and can extend to injury bordering upon pure accident, though an accident not due to (what lawyers call) an Act of God, but due at some stage or other to an act by Smith, a stage which may be a very remote cause indeed. The last point also permits the type-difference between deliberate and non-deliberate injury to be stated in crisper if bolder terms. While intentional harm almost always involves an immediate agency, negligent harm can, and very often does, result from much more indirect cause. Indeed, it is precisely this 'immediacy' or 'directness' which holds together statements about deliberate injury; for whether we refer to fists or sticks or guns, these and all other injurious missiles are more directly definable by virtue of their being directly guided and controlled.

Involved in the same contrast between deliberate and non-deliberate harm is the avoidability of the acts concerned. In the case of deliberate injury, its avoidance cannot only be located but can also be prescribed. In the case of non-deliberate injury, however, avoidability can, if at all, only be stated within a limited context. Thus as regards so-called 'reckless' or 'dangerous' situations we can specify some preventive measures such as 'Do not drive very fast in crowded streets for you are bound to hit somebody' or 'Do not play with dynamite, it is liable to go off'. These statements are like instructions to packers or dockers ('Handle with care'); the persons handling it are put on notice that the stuff is explosive or fragile. In these cases, moreover, the dispositional qualities of certain things are matter of common knowledge, if only because of their recurring patterns or frequencies. The above instructions 'Handle with care' or 'Do not

'drive too fast' can operate like 'inference-tickets': they tell us what to do, by telling us about certain risks, whether any harm actually ensues or not. Yet such 'reckless', 'dangerous' or 'risky' situations apart, there remains a much larger variety of cases for which no preventive measures can be prescribed, simply because the consequences of many negligent acts are both less known and less predictable than the breakage of glass or the explosion of dynamite. Again, even if we did know all about acts and their consequences, the course of the latter might still be too complex or too long. In illustration, consider the well-known legal case in which stevedores while unloading a ship dropped a plank into the ship's hold filled with oil vapour. The plank hit something, touched off a spark which caught the vapour, which put the ship afire. The court regarded the stevedores as negligent in dropping the plank and held them liable for the whole injury. What concerns us is not the legal decision itself, but the problem of making rules for this sort of case. Thus what sort of preventive measure could here have been prescribed? Could one say: 'Do not let drop planks which might set off sparks, which might catch vapours, which burn a ship?' This prescription would remain quite unsatisfactory in two respects. Though seemingly getting at the root of the trouble by specifically locating the two danger-spots (vapours and sparks), our rule would still not make things completely safe; for even such a notice as 'Watch out for vapours every five minutes', might not prevent vapours in the last minute or so. Secondly, the planks might slip rather than drop, either because of rain or because a new stevedore is on the job. Suppose then we made a rule: 'Do not let new stevedores handle wet planks, etc.' Yet, again, this would not really help, for even an old hand could easily let a plank slip because of sweaty hands or new slippery shoes or any number of things. Hence it seems more or less impossible to enumerate all the possibilities that might 'cause' damage or injury in such an indirect way; and even if not actually impossible, the enumeration of the relevant causes would be futile since their regulation would be impracticable anyhow. We would, obviously, need an enormous number of preventive rules just for one situation, yet a situation possibly extremely infrequent or perhaps never to recur. The cost and the delay of such an elaborate system of control would make stevedoring so slow and expensive as to make it economically worthless work: it would be quicker and cheaper to move mountains instead.

Since situations as the above are beyond the reach of manageable prescriptions or specific rules, yet since we also continue to ascribe

responsibility (or legal liability) for the harm they show, these ascriptions will tend to employ such general concepts as 'negligence' or 'fault' or 'duty of care'. We tend to say that (*e.g.*) the stevedores were to blame or liable because they negligently allowed planks to slip, etc., though the word 'negligent' will here be lost in a mass of unspecified detail. And we can now understand why a concept like 'negligence', and statements employing this and similar concepts, are inevitably defeasible. The defeasibility arises because no descriptive formula can contain or cover all the conditions ascribing responsibility. We could perhaps devise a formula for limited circumstances, but thus limited the formula would never indicate the whole range of facts over which the concept of 'negligence' could stretch or, as it were, could hover.

Let us take a final look at a typical action-statement like 'He did it'. Ascribing responsibility for what 'he did', will not be a uniform judgment, but will be as many judgments as there are cases. Still these judgments or ascriptions fall into two main groups: such like 'I expressly told you not to hit him', and such like 'You are responsible because you were negligent'. Though we can now distinguish between statements or rules concerning deliberate and direct acts and statements about non-deliberate indirect harm, both types of rules are (enough has been said why) logically ascriptive. Moreover, while one type of ascription ('He did it deliberately') is in the closest correspondence with the facts (a correspondence which indeed looks, but is not, one of entailment), the other ascription ('He did it negligently') stands in much looser relation to its factual background. And this, as we have seen, because our judgments are not, and cannot be, laid down prescriptively and in advance; being defeasible these rules such as they are will be announced *ex post facto*. Better still, the latter statements will always remain judgments, and will never develop into rules that prescribe or instruct. In sum and in conclusion, we are left with two types of ascriptive statements: (i) ascriptions that can form the subject of definite commands or prescriptions; and (ii) ascriptive statements that can never constitute such true commands since they incorporate defeasible concepts. And to the extent that I have shown this, I have, I think, made good my original claim that while the logical nature of ascriptions does not depend on a notion of defeasibility, the notion itself can, properly located, be very useful indeed.

VI.—ON THE ELIMINATION OF SINGULAR TERMS

BY MANLEY THOMPSON

I

IT would seem that one cannot learn ostensively (by direct confrontation) the use of a general term or predicate like "red" unless he can correctly interpret certain demonstrative signs. We may point to a variety of red objects and utter the word "red" in each case, but it will not help a person to learn to use "red" if he is unable to get the reference of the pointing. We must do more than simply confront him with a situation in which there is a red object; we must also indicate by some means which object among others in the situation is red. The process of learning to use "red", in other words, is a process in which one must come to understand what amounts to a statement like, "This is red". It is only after one is able to understand such a singular statement that he can get the point of a generalized statement like, "Something is red". The latter is clearly a statement we would expect some one to understand after, but not before, he has learned the use of "red" and hence has come to understand a statement like, "This is red".

This priority of the singular statement seems to mitigate any claim that we can have a language entirely free of singular terms or demonstratives. Though we can always in theory say, "There exists something which is uniquely so-and-so, and it is red", instead of, "This is red", we have eliminated the demonstrative only by introducing the predicate "so-and-so". This further predicate itself must have been learned with the help of demonstratives, either directly or through other predicates which were so learned. There is ultimately no escape from the recognition of demonstratives. Even though we can always paraphrase any given use of a demonstrative by a statement which contains a further predicate but no demonstrative, the paraphrase makes sense only against an ultimate background of demonstratives. For the sake of neatness in certain areas of logic we may claim the paraphrases are ultimate and ignore the formulations which contain demonstratives, but it is folly to make the much stronger claim that the paraphrases render these formulations superfluous for any part of the philosophy of language.

The foregoing is what I take to be the core of Strawson's

criticism of Quine's elimination of singular terms.¹ An obvious line of counterattack is to argue that the proposed defence of demonstratives rests on facts irrelevant to the issue of the eliminability of singular terms. I want to explore this line of counterattack, and at the risk of distorting both Quine and Strawson to present considerations which I believe are valuable in their own right for the philosophy of language.

II

The claim that all the predicates of a language can be learned with no help from demonstratives, in any sense, is of course very different from the claim that, once the predicates are learned, whatever can be said by means of them can be said without the use of singular terms. Quine is hardly to be taken as making the first claim nor is Strawson's criticism so wide of the mark as to assume that he is. Yet Strawson's main point turns on the fact that one of the things we can say by means of predicates is how predicates themselves are learned. Thus, with respect to Quine's proposal that, "Instead of treating the ostensively learned word as a *name* of the shown object to begin with, we treat it to begin with as a predicate true exclusively of the shown object"; Strawson asks, "How, in the linguistic reconstruction of Quine's supposed learning-situation, is '*the shown object*' to be represented?"² It surely cannot, Strawson continues, be represented by "the learned predicate corresponding to the proper name". No reason is given for this contention and Strawson apparently considers the point obvious. But it depends on what is meant by "linguistic reconstruction". It is one thing to represent the shown object in a way that would make sense *within* the learning-situation, i.e. in a way that does not presuppose knowledge of the predicate to be learned; and it is quite another thing to represent it in a way that makes sense only when *talking about* the learning-situation, i.e. in a manner adequate for a description of the situation though incompatible with the learning supposed to take place within the situation. Thus, there is nothing wrong in saying, "He learns what the predicate 'is-John-Doe' applies to by being introduced to the individual who is-John-Doe", when the intention is simply to describe a learning-situation. But when the intention is to tell some one what "is-John-Doe" applies to, it obviously will not do to say merely, "'Is-John-Doe' applies to the individual who is-John-Doe". In this case, John Doe must be represented ultimately with the aid of a

¹ Cf. MIND, lxxv (1956), 433-454.

² Loc. cit. p. 447.

singular term or demonstrative, the reference of which will be clear to the learner.

Now, by "linguistic reconstruction" Strawson can hardly mean a representation of the shown object within the learning-situation, yet it is only here that the impossibility of using the learned predicate seems obvious. Strawson's point, which I do not find obvious and which has to be gathered from other remarks in his article, seems to be the following. Even when our intention is merely to describe a learning-situation, we cannot get rid of demonstratives or singular terms entirely, because without the possibility of them our description would be senseless. "He learns what 'is-John-Doe' applies to by being introduced to the individual who is-John-Doe," makes sense as description of a learning-situation only in so far as we assume that, in the situation, John Doe can be represented by something other than "is-John-Doe". We in fact imagine a person being confronted with John Doe and hearing something like, "This is John Doe" or "I am John Doe". Hence, though the shown object need not be represented in the description by something other than the learned predicate, the possibility of its being so represented is needed to make sense out of the description. Thus (and here is the crucial point), one cannot in a language which contains predicates but no singular terms describe how predicates are learned. To be sure, we can imagine some other predicate in place of "is-John-Doe", but the application of this further predicate must also be learned, so there is no escape ultimately from assuming singular terms that are not predicates.

The final twist in Strawson's argument is then to maintain that when we cannot describe in a language how predicates are learned, we cannot in that language make sense out of predicates at all, because we cannot describe them as applying to particulars. As he puts his thesis on the next page, "I have argued only that a language which did not allow of reference to particulars by singular terms would not allow of any kind of reference to particulars; that the conception of a particular would have no place in the description of such a language".¹

III

(i) The point I want to urge against Strawson is that he has a wrong notion of what is meant by "a language". Suppose some one were to say, "It is unnecessary to think of John Doe as represented in language by anything other than the learned

¹ Loc. cit. p. 448.

predicate ‘is-John-Doe’. We need only assume that the individual John Doe can be singled out by appropriate demonstrative gestures at a time when the predicate in question is uttered.” Would Strawson reply that this concedes the point because the gestures in effect constitute a representation in language? While I find nothing in the body of Strawson’s article which affords a clear cut answer, to judge from a footnote near the end this certainly would be his reply. The note reads: “I need perhaps scarcely emphasize the point that these [demonstrative] elements *need* not be represented by clearly detachable bits of symbolism which make use of the same phenomenal materials (*e.g.* speech-sounds) as the rest of the language; though it is convenient to assume that they will be.”¹

Now this position, as I see it, fails to distinguish between two fundamentally different kinds of demonstratives. Suppose some one asks me where is the University Chapel. Consider first the case where I say nothing but turn and point to the top of the chapel tower plainly visible above the trees from where we are standing. Consider next the case where I neither turn nor point but simply utter the words, “That is the chapel tower there above the trees”. Are both of these equally cases of using a language? A sign that they may not be is the fact we would normally say of the first case that I showed rather than told the person where the chapel was, while we would normally say of the second case just the reverse. This distinction between showing and telling, taken simply at its face value, admittedly cannot decide the issue, and the appeal to it raises difficulties which I shall consider presently. But first I want merely to state what I intend by speaking of “two fundamentally different kinds of demonstratives”. In a simple act of showing, like that supposed in the first case above, the demonstrative act is non-verbal, and I propose to argue that such demonstratives can be nonlinguistic as well. On the other hand, the demonstratives in the second case above are verbal and linguistic; they are part of the vocabulary of the spoken language and, as I shall contend, are fundamentally different from the demonstratives of the former type.

(ii) All demonstratives function to direct attention, but only those I have called nonlinguistic can do so merely by being physically related to the object to which attention is directed. In pointing toward the chapel tower my arm moves in the direction of the tower, and one who follows this movement with his eyes will eventually find the tower in his field of vision. In

¹ *Loc. cit.* pp. 451, n. 1.

other cases of pointing, the physical connection is more obvious. The movement of the arm may terminate with the index finger actually touching the object, or with the fist knocking against it. The object may be seized by the hand and waved back and forth. In each instance, to be sure, verbal utterances usually accompany the demonstrative act and in some sense supplement its function. But for the moment I want to ignore these supplementations and to concentrate on the act itself as demonstrative. The point I want to emphasize is that it can be in virtue of the physical relation alone that the act itself becomes demonstrative. One does not have to learn rules—to acquire habits—in order to become sensitive to the demonstrative force of the act, as he does in the case of linguistic demonstratives. In the natural process of perception the eye physically follows the movement of the arm, and there is thus a natural directing of attention which in itself constitutes the demonstrative function of the act perceived. I say "*a natural* directing" simply to underscore the fact that acquired linguistic habits play no part in the directing, which is brought about solely by the dynamics of the perceptual situation.

The isolation of nonlinguistic demonstratives is made extremely difficult by the circumstance that such demonstratives are readily associated with the linguistic supplements that usually accompany them. Thus, one would normally expect my pointing to the chapel tower to be accompanied by some such utterance as, "That's the chapel tower you see there". It is easy to think of my pointing as a kind of nonverbal though still linguistic adjunct to the demonstratives "that" and "there" which occur in the accompanying sentence. We are thus led to conceive of a sort of primitive sign language (nonverbal) which we all more or less learn in the process of learning and using our ordinary verbal language; and it is then, so it appears, through the learned rules of this sign language that various pointing gestures acquire their demonstrative significance. Now I have no intention of denying that such a sign language does in fact arise. I only want to insist that granting the presence of this sign language in no way conflicts with the fact that various acts of pointing do have a demonstrative function, which arises naturally from the situation in which the pointing occurs, independently of any linguistic rules.

(iii) I come now to the difficulties with regard to showing and telling. The issues are many and I shall confine myself to those which I consider strictly relevant to the points I want to make. A source of trouble lies in the fact that while showing is itself basically nonlinguistic, *i.e.* there is nothing about it which

necessitates a use of language, some of the most common cases of showing are showing by a use of language. One's linguistic utterances, regardless of what they are meant to tell, usually express or show feelings, attitudes, capacities, states of mind, and the like. Again, it is common to speak of showing (demonstrating) by verbal argument that something is the case. Now I suggest that what all these showings by a use of language have in common with other ways of showing is simply that they present rather than represent. When we say that his remarks showed his capacity, we mean that we regard making such remarks as an actual exercise of the capacity ; the remarks present his capacity to us. To say that an argument shows something to be the case is to say that the connection it shows between premises and conclusion presents the truth of the latter.

We may avoid becoming confused by the fact that showing, though basically nonlinguistic, is often achieved through a use of language, if we keep clear the distinction between presenting and representing. Confusion arises readily from the fact that the very same linguistic utterance may, depending on its context, present or represent. There may seem to be no difference between saying, "His remarks showed me that he had the capacity", and saying, "His remarks told me that he had the capacity". I do not deny that on occasion one of the verbs "to show" or "to tell" may be used in an extended or metaphorical sense which would make the two statements the same. But I want to insist that when both verbs are taken in their customary sense, there is an important difference between the two statements. One would normally use "show" when he takes the person's remarks as themselves an exercise of the capacity ; as noted above, the remarks in this case present his capacity. On the other hand, "tell" would normally be used when the remarks are taken as representing the capacity—as a sign or token of it. Now with "tell" it is proper to use not only the adverbs "clearly" and "vaguely" (as it is with "show"), but also to use (as it is not with "show") the adverbs "truly" and "falsely". Another source of trouble is thus the fact that to take the remarks as representing rather than presenting the capacity may seem to imply some element of doubt, since a representation, but not a presentation, although very clear may still be false. Yet the two statements we started with seem equally categorical, and one would be expected to use something like, "suggested to me that", instead of, "told me that", if he meant to leave room for doubt. But it is a mistake to suppose that because a representation may be false it is therefore wrong

to speak of it with the same categorical certainty as one speaks of a presentation. I may be just as sure that a man's uttering certain remarks in response to a hypothetical situation described in an interview is a representation of his capacity, as I am that his uttering these same remarks in an actual situation is a presentation of his capacity. To be sure, the man might always go to pieces in the actual situation and make the wrong remarks, and we would then have to say that he does not fully possess the capacity. His uttering the right remarks in the hypothetical situation was then a false representation, but it was none the less a representation, and it remains correct to say categorically that his remarks in this case told me he had the capacity. To say, "they merely suggested he had the capacity", makes reference to the reliability of the representation and not to the fact that it is a representation.

(iv) Let us return to the example of showing and telling the location of the chapel. Suppose that in the case where I point but say nothing, I actually point to the library tower instead of the chapel tower. Obviously I am misleading the person, but we hardly want to say I am showing him falsely where the chapel is. I am plainly showing him something, but it is not the object he inquired about. Falsity arises not from my act of showing taken by itself, but from the fact that this act is given in response to a question. As such a response, my showing the library tower tells falsely the location of the chapel. My showing in this context thus becomes for the questioner a representation of the location. Just as a linguistic utterance, which taken by itself is a representation, may become a presentation in an appropriate context, so an act of showing, which taken by itself is a presentation, may become a representation in an appropriate context.

Now if my showing becomes a representation of where something is, must it in some sense constitute a linguistic demonstrative? It is not necessary, we should note first, that the questioner interpret my pointing as part of a primitive sign language—as a nonverbal equivalent of "there it is". He might do so, of course, but he might also simply respond to my pointing as a nonlinguistic demonstrative which directed his attention to the tower because of its physical relation to that object. Even so, he would take this directing of his attention as telling him the answer to his question, and we must thus say that the mere act of my showing, though in itself nonlinguistic, has performed a function (*viz.* telling) which could also be performed by linguistic demonstratives and predicates. The directing of attention functions demonstratively, while the shown

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object is taken in the context as that to which the predicate in the question (*viz.* "university chapel") applies. The net effect, *i.e.* the telling, is the same as in the second case where I merely said, "That is . . ." etc. The difference between the two cases is simply that in the second I directed his attention by linguistic demonstratives, instead of acting in a manner that would physically direct his attention to the object. It remains correct to say of both cases, then, that I told him where the chapel was, but only of the first case can we say that I did this by showing him something. The remark that in the first case I showed rather than told may still be correct if it is made merely to emphasize the fact that strictly, all I did was to show, even though the effect of my showing was, in the context, to tell.

(v) With this account of demonstratives and their function in showing and telling, we may say that Strawson has taken what I have called "nonlinguistic demonstratives", even when they occur in what is strictly an act of showing, to be part of "a language", though not using "the same phenomenal materials" as the rest of the language. While this may be the case when there is what I referred to as "a primitive sign language", I have argued that it does not have to be the case. It makes sense to speak of showing a person an object without any use of linguistic demonstratives, even though the effect of the showing is to tell the person where the object is. But then, it also makes sense to speak of showing a person, without any use of linguistic demonstratives, an object to which a certain predicate applies, even though the effect of the showing is to tell the person that the predicate applies to the shown object. There is no need to assume that the shown object can be represented in the learning-situation by something other than the learned predicate, precisely because the object, apart from this predicate, need not be represented at all but can remain simply presented or shown. I point to an object and say "red". My pointing may direct attention to the object solely by means of physical relations with it, and the pointing together with my utterance of "red" has the effect of telling a person that the object is represented by "red". The situation is exactly analogous to the example of the chapel, except that in the latter it was the person to whom something was shown rather than the shower who uttered the predicate. My questioner asked in effect. "Where is the object to which the predicate 'university chapel' applies?" I answered by showing him an object, and since the predicate by which the object is to be represented was already understood in the context, it was immaterial whether my showing was

accompanied by a further utterance of this predicate. It makes no difference with respect to the functions of showing, telling, and representing if one does not ask a question but simply has his attention directed to an object at the same time that he hears the person who is directing his attention utter a predicate.

I conclude, then, that the fact that at least some predicates of a language must be learned by direct confrontation is no argument against the eliminability of singular terms. The confrontation can always be achieved nonlinguistically by acts of showing, and we can thus make sense out of predicates in a language containing no singular terms.

IV

(i) I suggested at the outset that Strawson's defence of demonstratives might rest on facts irrelevant to the issue of the eliminability of singular terms. The facts in question concerned the learning of predicates, and in so far as it has now been shown that the demonstratives required for such learning need not be linguistic, the irrelevance is obvious. In the remainder of this paper I want to present a defense of demonstratives which seems to me directly relevant to the issue.

When I point to an object and say, "That is red", the word "that" does not represent my pointing or any other demonstrative act I may perform. Rather, it represents the object by standing for or in place of the object. Linguistically, I predicate "is red" of "that", though clearly I do not mean to say the word is red but that what it stands for is red. Pointing and other nonlinguistic demonstratives serve to direct attention physically to the object for which "that" stands in a given context. "That" and other demonstrative pronouns are of course variable or egocentric demonstratives; what object they stand for varies with the speaker and the context, and it is common to employ nonlinguistic demonstratives to show a person the object when it is not apparent in the context that his attention has already been directed to it. Proper names are exactly analogous, except that their contextual dependence is less variable. Once we have agreed that "John Doe" is to stand for a certain individual, it makes no difference which one of us in whatever context uses "John Doe"—the object it stands for is always the same and there is no need to employ nonlinguistic demonstratives to show the object. Though, to be sure, when we talk to some one who is unaware of our agreement about "John Doe", we must show him or tell him by description what the name stands for.

(ii) From this account, it might seem that the phrase "linguistic demonstrative" is a misnomer, since such expressions have just been said to represent rather than to show or present something. But consider the statement, "That is a brown dog". Grammatically, we would say that the phrase "a brown dog" may stand for, designate, or name any object it characterizes. But in the given statement which object in particular does it stand for? It stands for whatever "that" stands for, and hence "that" has the grammatical function of showing (directing attention to) what some other word or phrase in the sentence stands for. By itself, apart from its rôle in the sentence, the demonstrative "that" neither stands for nor shows anything. But in sentences it does both. It stands for whatever it is shown to stand for in a particular case (as when one points and says, "that"), and yet in standing for an object it neither names nor characterizes the object but merely shows which object some other word or phrase in the sentence names or characterizes. It is of course unnecessary for this other word or phrase to represent by standing for as well as by characterizing. It may merely characterize, as when we say, "That is brown and canine". In this case, "that" shows which object in particular the predicate characterizes. "That" is a demonstrative, then, because it shows through the structure of a sentence what object some linguistic element other than itself stands for and/or characterizes, though it cannot do this without itself standing for the same object. Nonlinguistic demonstratives, on the other hand, do not show by standing for but by physically directing attention. Proper names are generally not called demonstratives, since they do more than merely show what some other linguistic element names or characterizes. To be sure, in "Fido is brown and canine" the proper name, as grammatical subject of the sentence, shows what object the predicate characterizes. But it does more than this; it also identifies the object independently of the particular context. The object is not taken as whatever the speaker on this occasion may be using a demonstrative pronoun to stand for; it is identified as the object which is a certain individual.

This distinction between the demonstrative and the identifying function of names is crucial for the point I want to make. In a sentence like "That is Fido", the proper name need not function demonstratively. It identifies an object, but it does not show what object some other linguistic elements stands for or characterizes unless the pronoun is relative rather than demonstrative. When it is demonstrative, "that" shows what "Fido" stands

for and not vice versa. "Fido" then has an identifying but not a demonstrative function. The identifying function is performed by names and descriptions, but never by linguistic demonstratives alone. In a sentence like, "That painting is something he has always admired", the subject has both an identifying and a demonstrative function, but the identification is achieved only with the help of a noun and not by a demonstrative alone.

(iii) Now, I want to argue that it is the demonstrative and not the identifying function of singular terms which presents a problem when we eliminate them by paraphrase. Take the example, "That is brown". When we replace "that" by a name or a description of the object it stands for—say, by "Fido" or by "the dog from next door"—we obtain an expression which performs an identifying function as well as the demonstrative function performed by "that". It is then this identifying function alone that is preserved when we go on to eliminate the singular term by paraphrase. The result will be something like, "There is something which is-Fido and with which anything which is-Fido is identical, and it is brown". The important thing here is that "there is" be taken in its existential sense, so that "there" is not a demonstrative. "Is-Fido", of course, must be taken as a predicate and not a name. In this way, we preserve the identifying function in the characterization given by the predicate, but we so alter the structure of the sentence that no part of it performs a demonstrative function.

The question, then, with respect to the elimination of singular terms is whether the demonstrative function is theoretically superfluous. Even though the function seems indispensable in practice, is it still at least possible for us to state everything we want to state without it? Before examining the question, two points should be noted. First, the unqualified interchangeability of names and descriptions is not at issue. It may or may not be the case that we should regard proper names as nothing but abbreviations of descriptions. This is another question, and is not at stake here. The relevant point is simply that demonstratives, names, and descriptions can perform the demonstrative function equally well, and hence may be interchanged without loss of this function. The second point to be noted is that the meaning of fictitious or pseudo descriptions and names (*e.g.* "the present king of France" and "Pegasus") is also not at issue. Such expressions present a special problem because they either do not perform a demonstrative function at all, or they do not do so in the way that ordinary names and descriptions do. The issue here is the dispensability of expressions which do behave

demonstratively in the usual manner, and how we are to account for the special cases is a further question to be considered after and not in conjunction with the present one. The fact that we might avoid the problem of these special cases if the demonstrative function were always dispensable is by itself no argument for the dispensability.

(iv) Let us now turn to the question. Suppose I wish to tell some one that a certain object is poisonous. We might say roughly that I do this by first identifying the object for him and then telling him that the identified object is poisonous. What do we mean by saying, "I identify the object for him"? Perhaps I note that his attention is already directed to the object. I observe him looking at it intently, and I merely say, "That is poisonous". While I thus succeed in telling him the object is poisonous, I can hardly be said to have identified the object and then to have told him it is poisonous. There is simply no need for identification in the context supposed. He of course takes my "that" as standing for the object he is scrutinizing, and through its rôle in the sentence "that" shows him which object "poisonous" characterizes. To be sure, I might have used a descriptive phrase in place of "that", and said, "The vine with three-leaved clusters is poisonous". In this case, I have used an expression which identifies by predicates which object it stands for in the rôle it plays in the sentence. The identification is hardly necessary in the context supposed, but it provides a kind of insurance against my being misunderstood. (He might not have been looking at the vine but at an insect on one of its leaves.) The identification given is likely to prove unambiguous in the context supposed, but in another context I would probably have to add further predicates, and say, e.g. "the vine with three-leaved clusters growing along the north edge of the field". In such a context, the mere demonstrative "that" by itself would almost certainly fail to do the job and some identification becomes a necessity.

How much identification is required, how much I have to guard against possible ambiguities in my description, depends on the extent to which my auditor's attention has already been directed to certain objects. In practice, there are usually certain objects which it is reasonable to assume have already come to the auditor's (or readers') attention, and among which is the object I want to say something about. My identification then suffices if it distinguishes this object from the rest. When we are standing on a hill overlooking a well marked field, I can reasonably assume that being in this situation is enough to direct my auditor's

attention to the various objects of vegetation in the field. Then my description, "the vine with three-leaved clusters growing along the north edge of the field", should clearly single out from the others the object I want to label poisonous. As an extreme case, we might imagine a situation in which I resorted to something like, "the vine with three-leaved clusters growing at precisely latitude north, so many degrees, minutes, and seconds and longitude west, so many degrees, minutes, and seconds". In this case, I would be assuming that my auditor's situation is so unspecified that it can reasonably be taken as nothing more than the situation of being on the earth, and hence my identification must suffice to single out the object I intend from all others on the earth. But then, as the extreme limiting case we have a situation about which nothing whatsoever can be assumed, so that my identification, if sufficient, will distinguish the intended object from all other existing objects.

In this limiting case, as opposed to the others, we have a situation in which it is appropriate to begin with the existential "there is" and to dispense with the demonstrative function.¹ When I use the normal idiom for description and say, "The so-and-so is . . .", I am assuming that my auditor is in a situation where he can determine what object I intend the phrase "the so-and-so" to stand for. I am assuming that among the objects to which his attention has been directed, there is one and only one which is so-and-so. But it makes no sense to say his attention has been directed to all existing objects. One's attention is always directed to certain objects as distinct from others, however numerous and various these certain objects may be. Hence, in the limiting case I express what I mean when I use the cumbersome "there is something which . . . and with which anything which . . . is identical" rather than the customary "the so-and-so". I am saying that among all existing objects there is at least and at most one which . . .

Yet, one may object, is this not in effect what we always do when we use a phrase like "the so-and-so"? When I say, "The vine with three-leaved clusters growing along the north edge of the field is poisonous", do I not intend to single out one, and only one, object from among all existing objects and to say that it is

¹ Metaphysicians and theologians in their talk about God and His creatures certainly claim such a context. In what respects, if any, such talk makes sense, is not at issue here. The point is simply that in this context, at least, the demonstrative function is inoperative. A sentence like, "God is the supreme being", is to be understood as, "There is something which is supreme and with which anything which is supreme is identical, and it is-God".

poisonous? If it turned out that more than one vine answered this description, my utterance would in some sense have failed in its purpose, even if we do not want to say that it resulted in a false statement. But this is not correct. The existence in some other field of another vine which answers exactly my description in no way affects the success of my utterance, for its success does not depend on my distinguishing the object I intend from all other existing objects. It depends only on my distinguishing this object from the others to which my auditor's attention has been directed in virtue of his being, along with me, in the situation which prompted my utterance. To be sure, my utterance fails in its purpose when my identification proves insufficient to achieve this required distinction, but this is quite different from saying it fails whenever there exists another object answering my description.

The foregoing remarks, moreover, apply even when the predicate is one formed from a proper name, as "is-Fido". It may or may not be the case that a proper name, used as a proper name, achieves a kind of identification not achieved by descriptive phrases performing a demonstrative function. Since the question concerns the elimination of all expressions performing such a function, and since this elimination is claimed for proper names only in so far as they are first reduced to descriptions, it is only as so reduced that they are relevant here. But then, whether I say "the something which is-Fido" or "the something which is canine and from next door", I either do or do not intend the descriptive phrase to hold in the limiting case. If the former, I should drop the definite article and begin with the existential "there is"; if the latter, I should retain the normal descriptive idiom.

(v) The demonstrative function, then, is not merely dispensable in the limiting case—it is precluded by the very nature of the case. I misuse language when I say, "The so-and-so is . . .", and mean thereby to single out one object from among all existing objects. I succeed in saying what I intend in this case only when I begin my sentence with a phrase like the existential "there is", and refrain from using expressions which, by the structure of the sentence, are designed to perform a demonstrative function. It is in recognition of precisely this fact that we distinguish between the existential and the demonstrative "there is". Although the structure of the phrase would allow it, it makes no sense to construe "there is something which" as meaning, "there, among all existing objects, is something which"; in order to make sense out of the phrase, we must either construe

it as meaning, "there, among objects now drawn to your attention, is something which", or we must construe the "there is" as existential and not demonstrative.

The demonstrative function is thus not theoretically superfluous. We cannot eliminate it, where its occurrence is appropriate, since any paraphrase we might use makes sense only in a context where the demonstrative function is precluded and not merely superfluous. The point may now be summarized as follows. When I say, "The so-and-so is poisonous", I mean to assert that some particular object is poisonous, and the truth of my statement depends on whether this object is in fact poisonous. I use the predicate "so-and-so" to help my auditor identify the object I intend, and I prefix the definite article "the" to this predicate because I assume that among the objects to which his attention is directed by virtue of the situation we are in, there is only one object which is so-and-so. But this assumption has to do with the effectiveness of my communication and not with the truth of my statement. If the assumption is incorrect and my auditor wrongly identifies the object, my utterance fails in its purpose of communication. But the statement I made can still be true, since the object I intended may in fact be poisonous. While I have used the identifying function of "so-and-so" in making my statement, the statement that there is at least and at most one object which is so-and-so does not thereby become part of what I state. Yet the latter is exactly what happens when I eliminate the description by paraphrase, using the existential "there is". I then make two statements in conjunction, the first being in effect that "so-and-so" uniquely identifies a certain object, and the second, that the object so identified is poisonous. In this case, what I state is clearly false when the identifying function fails. And since the function is here asserted with respect to all existing objects and not merely with respect to those coming to the attention of my auditor, it may well turn out that my original utterance succeeds in its purpose of communication as well as makes a true statement, while the alleged paraphrase makes a false statement.

We cannot, then, state everything we want to state and eliminate by paraphrase all expressions with a demonstrative function. I want to state simply that a certain particular object is poisonous, and when I believe that a pure demonstrative like "that" will not get my meaning across, I use instead an expression which performs an identifying as well as a demonstrative function. In using such an expression I do not make the additional statement that its identifying function holds with respect to

all existing objects. This further statement is something I have no intention of making, and if the proposed elimination of expressions with a demonstrative function forces me to make it in conjunction with what I want to state, then such expressions are hardly shown to be theoretically superfluous.

V

This defence of demonstratives seems to me relevant to the issue of the eliminability of singular terms because it is based on the rôle which linguistic demonstratives play in enabling us to state what we want to state. The defence does not, as I have argued that Strawson's does, confound the issue by appealing to the indispensability of nonlinguistic demonstratives in learning the use of predicates. Quine's position, as I have interpreted it, makes no claim to the dispensability of nonlinguistic demonstratives. In a language which contained predicates and variables but no demonstratives, we could make sense out of the predicates because we could always in effect tell some one how ostensive predicates are used by showing him an object to which they applied. But then, we could also in effect tell some one a certain object is poisonous by showing him the object and uttering "poisonous". I might lead my auditor by the hand down the hill, across the field to the north edge, kick the vine, and say "poisonous". However inconvenient and practically intolerable, this is at least possible, and in such contexts it seems to me we must admit that linguistic expressions with a demonstrative function are theoretically superfluous.

I have deliberately ignored the complications which would be raised by phrases like, "the prime number between 5 and 11", "the Senate of the United States", and "John Doe's Oedipus complex". The situation here is clearly different; and I have stayed with examples of ostensive predicates, first, because Strawson rests his criticism on issues raised by such examples, and, secondly, because I believe the sort of elimination of singular terms proposed by Quine is shown to be inadequate by these examples. If I have understood Quine's thesis, it is a proposal to eliminate linguistic demonstratives by using another *linguistic* device, *viz.* that of quantified variables. With the defense of demonstratives I have suggested, this proposal must be rejected for the examples considered, except when the context is what I have referred to as "the limiting case".

VII.—DISCUSSIONS

DETERMINISM AND RATIONAL BEHAVIOUR

In his 'Determinism' (MIND, 1957, pp. 28–41) Mr. A. C. MacIntyre is concerned to attack the contention "that determinism and a belief in human responsibility are really compatible" (p. 41). Let us label this "the Compatibility Thesis". Selecting me as a recent spokesman, he develops his argument mainly in opposition to 'Divine Omnipotence and Human Freedom' (*New Essays in Philosophical Theology*, S. C. M. Press 1955, pp. 144–169). In this argument his chief contribution is to suggest an equation between free behaviour and rational behaviour. "Behaviour is rational—in this arbitrarily defined sense—if, and only if, it can be influenced or inhibited by the adducing of some logically relevant consideration" (p. 34). The crucial point is: "Rational behaviour is defined with reference to the possibility of altering it" (p. 35). Hence: "to show that behaviour is rational is enough to show that it is not causally determined in the sense of being the effect of a set of sufficient conditions operating independently of the agent's deliberation or possibility of deliberation" (p. 35). The purposes of the present note are: first, to show that to accept MacIntyre's equation would not be inconsistent with maintaining the Compatibility Thesis; second, to show that it is his ambiguous use of a crucial term which suggests to him that it would be; and, third, to examine three other mistakes which perhaps also play some part in reinforcing this suggestion.

(1) The three key sentences quoted will already have hinted that something is going wrong. For MacIntyre is concluding that to say that behaviour is rational, in his sense, or free is inconsistent with saying that it is "causally determined *in the sense of being the effect of a set of sufficient conditions operating independently of the agent's deliberation or possibility of deliberation*" (italics mine). This is his reason for rejecting the Compatibility Thesis: "There is no way out in arguing that determinism and a belief in human responsibility are really compatible. Whatever else is uncertain in this area of argument, of the genuine existence of the conflict that creates the whole problem there can be no doubt whatever" (p. 41). But MacIntyre's very strong sense of "causally determined" is of course not at all the weak, and surely more usual, sense of "determinism"; in which it has been held that determinism and freedom are not necessarily exclusive. The determinist, in this weak sense, believes only that events are predictable in principle, are determined by laws; and to add the epithet "causal" is to add the claim that these laws are all susceptible of causal interpretation—fortunately it is not necessary to pursue here the question of what precisely this involves. He is not thereby committed to fatalism, to holding that whatever people decide it makes no difference. For it is entirely open to him to

adopt what MacIntyre and I are in fact agreed is the manifestly true view, that human deliberations and decisions do frequently determine what happens : so long as he never denies that these are in their turn determined. By contrast, anyone who was a causal determinist in MacIntyre's strong sense just would by definition be a sort of fatalist, committed to precisely that implausible contention that the deliberations of agents are wholly ineffective and idle. The consequence is that, even if MacIntyre succeeds in showing that causal determinism (strong sense) must exclude freedom, he does not, as he thinks, dispose thereby of the Compatibility Thesis.

(2) It is hard to believe that MacIntyre can be making such a simple mistake. Yet that he really is becomes clear when we examine his uses of "determinism".

(a) In explaining in Part I the "precise contemporary importance" of the problem he remarks : "it is paradoxical that refutations of theoretical determinism—of Laplace's dream, for example—which satisfy most contemporary philosophers have been propounded and accepted in a period in which for the first time there has been accomplished what the opponents of classical determinism most feared" (p. 28). What they most feared was not the progress of the physical sciences : "but psychology and the social sciences. Sufficient has been already achieved in these sciences to make it clear that we can expect from them ever increasing success in explaining and predicting human behaviour" (p. 28). Here surely we are dealing with determinism only in the weak sense. For there is no reason—or, at any rate, no reason has been given—why either classical determinism in general or explanation and prediction in the human sciences in particular must involve the complete causal inefficacy of human deliberation. It is, however, significant that MacIntyre proceeds to cite as his examples three currently fashionable psychological hypotheses all of which do suggest that there may be rather more cases even than we had already thought where "the adducing of some logically relevant consideration" (p. 34) would be powerless to affect behaviour.

(b) In epitomising in Part II "the logical root of the trouble" with my argument he writes : "Flew tries to show that determinism is necessarily false. For on his view the determinist would be arguing that what is a *prima facie* free act is really unfree, whereas what we mean by 'a free act' is 'an act that is *prima facie* free'" (p. 32). These are extraordinary statements : for on his own previous, and perfectly correct, account of my position I was attempting to show : "that an act can be free and praise- or blame-worthy and at the same time causally determined" (p. 30); that "there is nothing about such an act which is incompatible with its being the predictable outcome of a causal sequence" (p. 31). MacIntyre's apparent *volte-face* becomes intelligible on the assumption that here "determinism" has to be taken in his strong sense. For my contention was, applying the Argument of the Paradigm Case to the

concepts "of his own freewill" and "could have helped it", that the thesis that these have no correct application is only intelligible if it is false. While, in so far as the causal efficacy of deliberation is a condition of freedom, anyone who was a determinist in MacIntyre's strong sense would indeed be open to attack on these lines.

(c) In explaining in Part III the sort of argument which the concept "rational behaviour" is to be offered to meet "determinist" is again used in his strong sense or something like it; certainly not in the weak one. Thus he writes: "The list of features which must be absent from a free act is indefinitely long. . . . The determinist claims that one of these features will always be present in any *prima facie* free act" (pp. 33-34). If it was with determinism in this sense that the Compatibility Thesis was concerned, then indeed it would be absurd beyond dispute; and there would be no need for even a short article to display its indefensibility.

(d) In considering in Part IV the "counter-attack" which "may be launched by the determinist" (p. 37) things are less clear-cut. The enemy sometimes advances boldly, a determinist in the strong sense. Sometimes by subtle manoeuvres he attenuates himself, becoming a determinist only in the weak sense. And sometimes his tactical virtuosity is such that it is hard to decide what his position now is.

Thus in the "second stage of the determinist argument. . . . It might be suggested that there are empirical grounds for believing that we can always be mistaken about rational behaviour, that it may on the surface be influenced by rational considerations and yet be in fact wholly determined by antecedent causes" (p. 38). Here obviously we are dealing with a strong determinist.

But "the first stage" was "to point out that in the widest sense of the word 'cause' the giving of a reason may function as a cause" (p. 37). Here apparently we are dealing with a weak determinist. For presumably the point of this move precisely is to indicate that a (weak) determinist is not committed to denying the efficacy of deliberation, and hence that of "the adducing of some logically relevant consideration" (p. 34). He is noting: "that the giving of a reason is not a reason, is always in fact a physical event of a certain kind, the uttering of sounds or the writing of letters, and is therefore admirably adapted to function as a cause" (p. 37).

In the third stage we seem to start with a weak determinist but to end trying to deal with him as if he were a strong one. For he produces: "what certainly is his strongest argument. . . . Given a detailed knowledge of the subject's learning history and achievements . . . when the subject was confronted with . . . good reasons for behaving in a particular way, we should be able to predict on the basis of laws covering both predisposing and exciting causes how the subject would react" (p. 39). But MacIntyre's last word in reply is: "that whereas the contention that my behaviour is determined by causal factors is normally taken to mean 'determined by causal

factors as contrasted with rational appreciation, etc.', here 'causal factors' have nothing to be contrasted with and hence the expression 'determined by causal factors' has been evacuated of its customary meaning" (p. 40). Against a strong determinist this argument might be knock-down decisive. Against a weak one it has no force at all. For a weak determinist is committed to denying only unpredictability in principle; not the efficacy of rational appreciation. So his doctrine would be threatened with evacuation of meaning only if no sense could be given to the former notion. With the latter he has no such direct concern.

(e) Summing up in Part V it looks for a moment as if MacIntyre is at last going to make the distinction which I claim to be crucial: "the determinist must either interpret his own thesis in a wide or in a narrow sense" (p. 40). The latter seems to be the same as the one I have labelled strong. He suggests that we have sufficient reason to know that determinism in this sense is false. Here—perhaps to his surprise—we are completely in agreement. Yet the nature of the wide sense remains obscure. For the only further elucidation offered is this: "If the determinist however interprets his thesis in the wide sense, then he obliterates that contrast between determined behaviour and rational, responsible behaviour on which his case essentially rests" (p. 40). This is obviously intended to recapitulate the argument deployed against the third stage of the determinist's counter-attack (see 2(d) above). And, as we have seen already, while the suggestion which he is there presented as sponsoring makes him a weak determinist, the last part of MacIntyre's reply depends for its force and relevance entirely on the assumption that he is a strong one. Finally, in the two concluding sentences (quoted in 1 above) emphatically rejecting the Compatibility Thesis, MacIntyre is presumably switching back unequivocally to the weak sense of "determinism" there in point.

(3)(a) In his prefatory Part I MacIntyre claims: "That this is an important problem no one presumably disputes. . . ." (p. 28). Part II begins: "Some contemporary philosophers, however, have tried to show that there is no real problem here and that if we only remove certain misconceptions the whole dilemma will be resolved" (p. 29). Part V ends: "There is no way out in arguing that determinism and a belief in human responsibility are really compatible. Whatever else is uncertain in this area of argument, of the genuine existence of the conflict that creates the whole problem there can be no doubt whatever" (p. 41). The first and second of these statements look as if they are contradictory, since surely no one who disputed the reality of a problem could concede its importance. But in fact I suspect a confusion only. For a problem can be important in many ways even though it is a matter of a "cluster of logical puzzles" (p. 28) and not of either ignorance as to what is the case or uncertainty as to what is to be done. This is presumably conceded by the application of this description to "'the problem of free-will and deter-

minism'" in the sentence next but one before the first just quoted. On the other hand there is a perverse and philistine usage in which real problems are contrasted with logico-philosophical, dismissed under the misleading label "pseudo-problems". There is also an idiom by which the man who thinks he has the answer may say that it is not a (real) problem; however genuinely those who do not share his illumination may be perplexed. But I will waste no space repeating what I have said about these idioms elsewhere.¹ It is enough to suggest that by insisting on fathering them onto the protagonists of the Compatibility Thesis MacIntyre may have helped to entangle himself. For perhaps one subsidiary reason why he mistakes every determinist to be denying that we ever act freely is this. He sees that the problem is important, in that the issues matter, and genuine, in that people are seriously perplexed by it. It would be easy to pass from this through these confusing idioms illegitimately to the conclusion that it must be genuine in the different sense of the third statement quoted; that is, as arising from the subsistence of a logical incompatibility.

(b) In his very fair summary of my argument MacIntyre writes: "Flew cites what he regards as a paradigm case of 'acting freely'. His example is that of two young people who decide to marry. They are not under pressure of any kind. . . . The importance of the paradigm case is that it is regarded as the kind of example by means of which the expression 'acting freely' would have to be taught" (p. 30). He then develops the idea that this is compatible with the possibility that the bridegroom was acting under post-hypnotic suggestion, and proceeds to the triumphant conclusion: "Our dissatisfaction with Flew's argument is heightened because the kind of case we have imagined might have been adduced as a paradigm case to explain what is meant by 'acting unfreely'" (p. 32). Indeed it might! If spokesmen of the Compatibility Thesis really were reduced to admitting that freedom and determinism are compatible only if "freedom" is so interpreted that acting freely is consistent with acting under post-hypnotic suggestion; then this certainly would suggest that freedom, in any ordinary sense, must be incompatible with determinism. I share, though for different reasons,² his dissatisfaction with at any rate my own sketchy applications of the Argument of the Paradigm Case to this problem. But this will not do at all. He has ignored the specification included in his own summary: "They are not under pressure of any kind. . . ." (p. 30). To this I think he would reply that I am not entitled to count hypnotic suggestion as pressure while refusing to admit other causal factors to the same category: "But Flew does not wish to exclude the presence of other causal factors such as glandular

¹ *Logic and Language*, i, pp. 4-6. *Essays in Conceptual Analysis*, pp. 12-14.

² See *Analysis*, vol. 18, in reply to J. W. N. Watkins' "Farewell to the Paradigm Case Argument".

states. . . . There is no relevant difference in logical status between explanations in terms of endocrine glands and those which refer us to hypnotic suggestion" (p. 31). But, first, there is a difference which, as I suggested in the original article (p. 153 and pp. 160 ff.), may be crucially relevant: with the latter there must always be at least one other person to share or bear responsibility; with the former this is not necessarily so. Second, we take it that the prehypnotized subject now cannot do otherwise, cannot be deflected "by the adducing of some logically relevant consideration" (p. 34). But it would be quite wrong to think because a man's glandular condition is part of the total sufficient cause of whatever he does that therefore he can do no other, and in particular that he could not be deflected from what he would otherwise do by that adducing of reasons which is impotent to affect the pre-hypnotized subject.

One Parthian shot. No one reading MacIntyre's paper would guess that the concept of post-hypnotic suggestion had played a central part in mine (pp. 160 ff.).

(c) In dealing with the third stage of the determinist counter-attack MacIntyre confesses: "I find it difficult to see how determinism could ever be verified or falsified" (pp. 39-40). This is apparently weak determinism, and we have tried to meet the difficulty (see 2(d) above). But the first part of his reason is also significant: "Suppose . . . that my behaviour is rational, that whatever (*sic*) strong reasons are adduced for acting in a certain way I act in that way, that I am infinitely flexible and resourceful in meeting new contingencies. Then no test will be available to decide whether I act as I do because it is the rational way to act or because it is the way in which my deeds are causally determined" (p. 40). This is true; but why expect such a test? It seems that either MacIntyre is himself assuming, or else he is taking it that the determinist is committed to saying, that such explanations answer the same question *and are necessarily competitors*. And such an assumption could be a part cause as well as one consequence of his taking "determinism" sometimes in the strong sense. So long as "determinism" is taken in the weak sense both assumptions are surely at least doubtful. I suggested in my paper (pp. 152-153), that both are in fact wrong.

ANTONY FLEW

ON EXPLANATION IN HISTORY

MR. Alan Donagan in his recent article in MIND (1957) on "Explanation in History" has raised some important issues regarding the nature of historical explanation. Primarily, Donagan is concerned to revoke the credentials of the "Hempelian Theory" which contends that historical explanations resemble the explanations of natural science in that they must be based upon the use of general laws. Donagan's position is the consequence of his belief that while there are in fact valid historical explanations, "the social sciences have not established any general laws".

It is our contention that Donagan has overstated his case. While his critique of the Hempelian Theory is in essence correct, in that not *all* historical explanations are "Hempelian", i.e. subsumptions of particular cases under general laws, Donagan is, we believe, wrong both in maintaining that there are no general laws in the social sciences, and also, *a fortiori*, in holding that such laws can have no place in historical explanation. On our view, Donagan's analysis of the role of laws in historical explanation is in part correct, and in part inaccurate and inadequate. It is the purpose of this paper to emphasize the strengths and to correct the weaknesses in Donagan's position.

I

Let us first consider the matter of the limits or limitations on the use of general laws for the business of historical explanation. No student of scientific method would, of course, undertake to question the undeniable importance of general laws for scientific explanation. But it is necessary to make two methodological observations which apply to the use of laws in explanations of any sort, and which must by consequence apply to history as well.

The first point is that wholesale insistence upon use of *general* laws in explanation leaves statistical laws out of account. Explanations can surely be based upon statistical law, such as insurance tables, and these are not deductive explanations using general laws.

The second observation is related to the logic of inductive reasoning. Carnap has argued that it is always possible to transform deductive explanations which use laws based upon empirical evidence into strictly inductive explanations for which no general laws are utilized. In his *Logical Foundations of Probability* (pp. 573-575) Carnap considers the question, "Are laws needed for making predictions?", and he demonstrates that the answer must be given in the negative. The same line of reasoning, however, applies *mutatis mutandis* to establish the analogous point with regard to explanations based upon the use of empirical laws.

These two considerations indicate that general laws are not requisite for explanations in general, and thus in particular, not

for historical explanations. So far, therefore, we must agree with Donagan's critique of the Hempelian Theory in its initial enunciation. But we must now take up the question : Do general laws play just the same sort of role in historical explanation as they serve in the natural sciences? Donagan answers this question negatively, because he holds that the social sciences yield "law-like" propositions, but not laws. The character and the role of these law-like statements requires explanation and analysis. We will return below to the matter of laws.

Donagan's conception of law-like statements derives from ideas propounded by Ryle in his *Concept of Mind*.

What of the statement, 'Those windows were brittle'? There is no doubt that to make that statement is to assert a general hypothetical proposition about those windows, which is, roughly, that if sharply struck or twisted they would not dissolve or stretch or evaporate but fly into fragments (p. 89). In some respects this hypothetical is open. . . . But in one respect it is closed : It mentions certain individual windows. Its form is not 'If anything is F it is G' but 'If these individuals should be F they would be G'. . . . The hypotheticals which are partly closed Ryle has given the name 'law-like statements' : they resemble general laws in being general hypotheticals ; they differ in not being completely open (pp. 123-124).

Ryle views as the pivotal characteristic of such statements the fact that they make reference to individual things or persons. However, something more fundamental than the reference to particular individuals is involved here, which the formulations of Ryle and Donagan alike miss. The reference to individuals is not actually the key facet of the sorts of restricted generalizations which Donagan appears to have in mind. Rather, such limited generalizations, i.e. law-like statements, explicitly or obliquely involve the use of *proper names*, and proper names can, of course, represent items of discourse of higher levels of complexity than individuals. The significant cases here are those of generalizations which are not the mere result of applying established general laws to certain particular individuals, e.g. universal properties of glass to particular window panes, Newton's laws of motion to the planets to derive Kepler's laws, or the like. The window example obscures the fact that the most significant law-like statements in the present context are those limited generalizations which hold in a certain particular application but for which there is excellent reason to think that they do *not* hold in general, e.g. the eating-habits of eighteenth-century English squires. In any event, it is the use of proper names that constitutes the defining characteristic of law-like statements.

This point has a direct and important bearing upon history. Historians do make generalizations—the military historian, for example, tells us that certain tactics in the use of cavalry in the massive land-engagements of the American Civil War proved particularly effective. Such a statement is no mere description, but has law-like

force, in asserting that if the military conditions and technology were again to be realized, the same tactics would again prove effective. Such historical generalizations, however, are formulated by explicit or implicit use of proper names: of individuals or groups of individuals, or periods of time, or nations or societies, of customs and institutions, of systems of technology, or the like. Thus historical generalizations, even if free from explicit use of proper names, introduce conditions which effect in an oblique manner the same delimitation of applicability.

While it must be granted that Ryle's view covers the case of many historical explanations, it is important that we also take cognizance of the fact that his analysis is not applicable without restriction because the law-like statements may be simply lower-level hypotheses—*i.e.* statements intended to be of a strictly general sort, but formulated under explicit limitations perhaps because the evidence that might warrant a widening of the area of generalization was not available or was not utilized, or because the investigator deliberately confined his attention to a particular area or period of time. This seems actually to be the case in Donagan's example of the plundering Danes who eventually settle the territory as opportunities for plunder diminish. Here broader investigation of the phenomenon of plunder might well show that this example can be assimilated to, and thus both derive evidential support from, and furnish evidential support to, some fully universal characterization of the behaviour of marauding peoples.

On the other hand, it could very well turn out to be the case that an examination of the evidence for a law-like statement would produce the conclusion that no extension of the limited generalization was warranted. Generalizations of the kind here under discussion are not universal in the sense that they are not valid for all times and places. Rather, their applicability may explicitly, or by reference to circumstances which *could*, but cannot reasonably be expected to recur, be limited within specific geographic and temporal bounds. The discovery of such restricted generalizations for the purpose of explaining historical transactions is perhaps the most central and characteristic task of the historian. Historians tend to formulate, not general laws, but restricted generalizations, limited by spatio-temporal considerations, but fully valid and law-like within them.

Similarly restricted laws can, of course, be formulated within the domain of natural science. Why scientists rarely do so is because for the most part they are not interested in such activity because their purposes are better served by the truly general laws which are available in their sphere. We do so in history because we *are* interested and also, of course, because of the frequently limited character of the available data. In this way the facts regarding the nature of man's interest in the materials of the past combine with practical limitations to shape the general confines of historical explanation.

The use to which Donagan puts the resort to law-like statements warrants comment. Ryle points out that we first make limited generalizations about individuals before we formulate laws. Donagan uses Ryle's discussion to support the contention that general laws *presuppose* limited generalizations or law-like statements, and thus to bolster his thesis that explanation in history is never Hempelian. But this wholly mistakes the force of Ryle's argument by confusing the *temporal* order of discovery (particulars to limited generalizations) with the *logical* order of explanatory reasoning (generalizations to particularizations). Instead of demonstrating that laws presuppose law-like statements, Donagan's argument shows no more than that empirical laws must be based on evidence.

To the extent of accepting, subject to indicated qualifications and amendments, restricted generalizations or law-like statements as important for historical explanation, we are in agreement with Donagan. But at this juncture we reach a parting of the ways, for Donagan insists that only restricted generalizations, and never truly general laws, can provide a basis for historical understanding.

II

Donagan argues that historical explanation does not use general laws and that "the social sciences have not established any genuine laws". Now if this last statement were true, then Donagan's thesis that historical explanation does not proceed by general laws would indeed be plausible. Some social scientists have in fact argued that the social sciences have not developed general laws. However, actual examples of general laws can be cited from several of the social sciences.

Psychologists have discovered that "norm-formation is more extensive in cohesive groups", and that "cohesiveness in groups leads to greater conformity".¹ A number of general laws have resulted from the work of anthropologists. Murdock, for example, has shown that "(all societies) recognize a system of culturally patterned relationship between kinsmen", and that "nowhere on earth do people live regularly in isolated families".² The same author has constructed "a universal law of sexual choice" which, although it does not lend itself to short statement, seems to be confirmed by an impressive amount of evidence. Similarly he has developed several laws regarding the normal order of change among the principal elements of social organization, and a general law pertaining to kinship terminology.

Professor Quincy Wright in his massive *A Study of War* (Chicago, 1942) has discovered that "balance-of-power policies . . . have tended toward polarization of all states about the two most powerful

¹ Michael Argyle, *The Scientific Study of Human Behavior* (London, 1957), 122, 127.

² C. P. Murdock, *Social Structure* (New York, 1949), 96.

of the group leading to serious wars involving all of them" and that "stability (in the state system) will increase and the probability of war will decrease in proportion as the number of states in the system increases". The economic historians in their turn have established the generalization that "every economic society prescribes its own standards of conduct".¹

Finally, consider Donagan's own discussion of the case involving the heavy influx of gold and silver into Europe during the sixteenth century. His argument rests on the point that, while the conditions of historical action (the rise in prices which followed) are to be explained by economic laws and are therefore Hempelian in form, the response made to the price rise is not so explicable but is derived instead "from the knowledge and character of the agents concerned". This is surely mistaken, for it is quite possible to frame a general economic law such as "when goods are scarce and gold is plentiful men tend to value goods more than gold" which makes it possible to explain the responses of the men involved.

The examples cited above tend to indicate that general laws covering various aspects of human behaviour not only exist but can prove useful to the historian in his search for more adequate explanations of why men acted as they did in particular historical situations. But in any event it should be noted that the case we are here endeavouring to make does not depend upon whether the laws that have been cited are correctly enunciated and wholly consonant with current evidence. The crucial point is that at least some conscientious social scientists have undertaken to formulate general laws within their fields. This of itself affords a strong presumption against attempts to rule out laws from the social sciences on methodological grounds. For it is an empirical question whether these laws are correct and adequate in the light of the evidence available and such questions ought never to be pre-empted on the basis of purely theoretical considerations.

III

Let us now review the position at which we have arrived and examine the bearing of the foregoing considerations for the nature of historical explanation and for the methodology of history. There are several points that we wish to make here.

As Donagan suggests, one of the main tasks of historical explanation is to explain past transactions "as a response proceeding from the knowledge and character of the agents concerned". What Donagan fails adequately to recognize is that two distinct explanatory resources are here involved: (i) responses proceeding from strictly general laws regarding human behaviour (*e.g.* of medicine or

¹ E. Lipson, *The Economic History of England* (London, 1943), vol. 2, LXXIV. Also Sir John Clapham, *A Concise Economic History of Britain* (Cambridge, 1949), chaps. iv and vii.

anthropology or psychology), and (ii) those proceeding from knowledge about the individual idiosyncracies of either (a) individuals, or (b) cultures, institutions, technologies, or the like.

Thus schematically three elements play a role in historical explanation.

- (A) Specific items of biographical or chronological fact,
- (B) Restricted generalizations—i.e. “law-like” statements limited in applicability by explicit or oblique use of proper names (of places, periods of time, systems of technology, or the like), and
- (C) The general laws afforded by the social and natural sciences.

In his eagerness to invalidate the Hempelian thesis Donagan is mistaken in denying the usefulness of general laws of history and the existence of such general laws in the social sciences. For an historical explanation might actually involve a mixture of all three types of the above propositions or it might, on the other hand, consist of some appropriate subset of these types.

Finally, it seems that the foregoing considerations have a significant bearing upon the methodology of history and the social sciences because, if correct, they serve to set these domains of enquiry apart from the natural sciences. It would seem that explanation in the field of human relations is not wholly reducible to a natural science using truly general laws because both in history and in the social sciences there arises a requirement for generalizations based upon time and place-bound particulars. Explanation in history and the social sciences can be furthered by the use of general laws, but cannot be exhausted by the use of such laws. The nature of man's interest in the past and the practical difficulties of the date together dictate the establishment of restricted generalizations which, when shaped by the hand of a master, provide us with a deeper understanding of the past and limited guideposts for the future.

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THE NECESSITY OF KANT

MR. ROBINSON, in an article in MIND (July 1958), blames Kant for what he takes to be muddles about necessary propositions. He says in the paragraph central to his criticism of Kant (p. 293) :

I regret to say that I think that the concept of a necessary proposition is now a muddle, and that this muddle began with Kant. I think that Kant's concept of a necessary proposition is nothing definite, but just a confusion of the four clear concepts of a necessary proposition which I have indicated. Kant's necessary proposition is none of these four necessary propositions precisely, but it is all of them confusedly.

I want in this brief note to suggest that at least part of whatever blame is to be distributed for any such muddles about necessary propositions should be allotted to commentators on Kant, such as Robinson himself. It may be true (and *part* of what Robinson says) that in Kant is to be found the origin of muddles of this kind, much as it may be true, as Balfour once implied, that in Christianity is to be found the origin of sin. But Robinson evidently wishes to say more than this, namely that Kant was muddled in his own account of necessity, and not merely the original occasion of others' muddle on the same subject. Since Robinson's conclusions in this paragraph are supported by reference to the ways in which Kant is supposed to have confused the four clear senses of 'necessary proposition', I shall consider his arguments one by one.

(i) Kant's necessary proposition is partly and confusedly the Aristotelian necessary proposition (p. 293).

The first argument in favour of this is to say that it is suggested by those of Kant's examples of necessary propositions which contain the word 'must' (or some such word, e.g. in German). For this criterion of a necessary proposition is simply that it should contain some such word as 'must'. This argument is simply absurd. There may be some senses of 'necessary proposition' in which it would be unnatural or impossible to express such a proposition by adding such a word (e.g. perhaps this second sense is such), but there are also many cases (among the first, third and fourth senses) in which this would be both natural and possible. That is, if I happen to believe that a proposition 'A is B' is necessary, it would be quite natural to express it by saying, sometimes, 'A must be B'. The fact that I might sometimes do so is quite irrelevant to the question of what I mean by 'necessary proposition'. The same fact certainly does not in the least suggest that I mean by 'necessary proposition' one which contains some such word as 'must'. It may be that Robinson has been misled here by his claims that the four senses are 'independent'; for the relation between the Aristotelian account and the others is somewhat more intimate than this austere word suggests. For one natural way either of expressing propositions said to be necessary in the other senses, or of expressing the propositions that

they are necessary, would be to satisfy this Aristotelian criterion. And for this reason if I do choose to express such propositions in this natural way, I will be satisfying this criterion, but I won't in the least necessarily want to say that it is in this sense that I use 'necessary' of propositions.

The second argument in favour of this first claim is that Kant's talk of statements which 'think necessity' ('B 4'), or 'contain necessity', or 'whose assertion brings necessity along with itself', 'more than suggests' that he was adopting this criterion. Robinson tries to support this claim by saying that 'When Kant said metaphorically that a statement "contained" necessity, he meant in part that it asserted necessity, i.e. asserted that all S *must be P*'. But no reason is given why we have to construe the metaphor as meaning 'the statement asserting that something is necessary' rather than as 'the statement asserting something that is necessary'. Even if the former construction is put upon Kant's metaphor it does not follow that he is subscribing to the second criterion. Two points serve to support the claim that this construction is anyway not certain. At B 100-101, where Kant discusses his 'modal' classification of judgments, although he links the propositions that p and 'p is necessary', he distinguishes them rather than identifies them with each other. Also at B 142, where Kant discusses how we should express the necessity that attaches to judgments, he explicitly says, of this kind of case, that in such a judgment 'I do not here assert that these representations (the subject and predicate) *necessarily belong to one another*, in the empirical intuition. . . .' In this case, then (and it is obviously enough a central one), Kant means by 'necessary proposition' something that could not legitimately or unambiguously be explained by such a form as 'A must be B'. For these reasons Robinson's argument seems to me to be inadequate. I conclude that unless he has some unstated evidence for his claim that this is what the metaphor precisely means (even though perhaps metaphors just do not *precisely* mean anything) there is no reason to believe that it is. Curiously Robinson himself appears to admit this on page 292, where he says of Kant's idea of 'necessary proposition' that 'He certainly did not understand it in the Aristotelian sense'. If I follow the argument aright, then this should have read, 'He certainly did not understand it only (or clearly or unambiguously) in the Aristotelian sense'. But the subsequent arguments are too weak to establish that Kant understood it at all (unclearly or ambiguously) in this sense.

- (ii) Kant's necessary proposition is partly and confusedly the universal proposition (p. 293).

Robinson claims in this passage that Kant was in part talking of necessary propositions when this meant only 'propositions of unrestricted generality'. In this way he tries to show that Kant was, in part at least, committed to this fourth sense of 'necessity' in which a proposition is necessary simply when it has the form of an

unrestrictedly general proposition. So far as I can see Robinson does not argue that Kant uses this criterion at all, but simply assumes that he does. If his ground for this assumption is the passage at B 4, where Kant discusses the twin criteria for a priority, namely universality and necessity, then this assumption is quite unjustified. It is, of course, true that in the passage Kant once speaks of unrestricted generality as a means of determining whether propositions are *a priori* or not. But of the five, direct or indirect, appeals to the universality criterion, no less than four refer not to 'unrestricted' but to 'strict' generality. Moreover Kant cannot, in this passage, have wished to say, as Robinson suggests, that any proposition that is unrestrictedly general is therefore necessary, since he explicitly provides an example of an admittedly unrestricted claim, 'All bodies are heavy', which is certainly not a necessary proposition. Since this example is just as explicitly stated to be a necessary proposition under Robinson's fourth test, it appears obvious that Kant was not talking of necessity in this way. What Kant appeals to when he talks of 'strict generality' is not a simple criterion of the (unrestrictedly general) form of a proposition, but a criterion for the authority in accordance with which a statement of such generality is legitimately made. A statement may, like 'All bodies are heavy', be unrestrictedly general, but it would be necessary or *a priori* only if there were a strict authority to make it in this form. That is, Kant's universal criterion for *a priority* turns on the question whether there is authority for a claim of unrestricted generality, and not on the question, which governs Mr. Robinson's fourth sense of 'necessity', whether a claim simply is expressed in an unrestrictedly general form. Since this is so, the assumption underlying Robinson's argument at this stage that Kant is, even partly or obscurely, relying on this fourth sense of necessity, seems to me to collapse, and to bring the argument down with it.

Robinson tries, however, to provide cases where Kant's ascription of necessity amounts only to the necessity which he claims is to be found in all universal propositions. The one example that he gives is the proposition that 'In all changes of the physical world the quantity of matter remains unchanged'. This proposition is either a version of the second part of the principle of the first Analogy in the second edition, or else derives from it. In this case Kant's 'proof' of the proposition derives from the proof of the first part of the first Analogy, namely that 'In all change of appearances substance persists'. Kant provides a very complex and elaborate argument in the first Analogy to establish the special truth of this principle. It is simply unwarranted to say, without any argument at all, that the only kind of necessity about this proposition is that if it were true it would necessitate each particular change to preserve the total quantity of matter. It must be remembered, too, that Robinson has to show not only that the proposition in question is necessary only in this trivial sense, but rather that Kant is partly

relying on this sense of 'necessary'. That anyone should seriously believe this is astonishing. Robinson believes it largely, I think, as a result of his failure to distinguish between Kant's account of 'strict' as opposed to 'unrestricted' generality. I conclude that even Kant's partial or obscure reliance on the fourth sense of 'necessary proposition' is by no means established.

(iii) The 'compulsory-belief' sense of 'necessary proposition'.

Robinson does not claim in this case that Kant was partly adopting this sense of 'necessary proposition'. It is remarkable, therefore, that Kant actually did adopt such a use, though he does not seem to have been especially confused about it. At B 851-852 Kant distinguishes between what he calls contingent and necessary belief, and it is clear that his account of the latter would come under Robinson's 'compulsory belief', and that it would be quite natural to speak in this way of a necessary proposition so believed. Nevertheless Kant does not confuse this account of necessity with any of the senses which are given to 'necessary' in the theoretical part of the Critique, since he restricts the former to what he calls the 'practical point of view', i.e. the spheres of 'skill' and 'morality'. This 'compulsory-belief' account of necessity has therefore nothing to do with the accounts given earlier in the Critique, and particularly in the Analytic.

The interest of this last example is primarily the light it casts on Kant's ability to discriminate between different senses of the same word. Throughout Robinson's attack on Kant reference is made to 'Kant's necessary proposition' or the 'Kantian sense of "necessary proposition"', as though Kant could only ever have meant to use this phrase in a single unequivocal way. This example (and there are others) shows that such an assumption is simply unjustified. Even if Robinson had been right in his attempt to show that Kant uses the term 'necessary' in different ways more evidence would be required to show that Kant was confused about this. But in fact I do not believe that Robinson has identified any of the important ways in which Kant does use the term. Nor is this surprising, since he restricts his attempt to 'collect this Kantian sense of "necessary proposition"' almost entirely to the Introduction to the Critique, and particularly the passage B 3-5. This may be only to follow Kant's own naively optimistic view (B 19) that something is to be gained from a preliminary general account of the Critique; but that this is a naive view is clear from the muddles which commentators get into when they concentrate only on this preliminary passage. Kant's account of necessity is very complex, and can be constructed only from a careful interpretation of the whole Critique, particularly the Analytic. Robinson takes himself to be opposing a contemporary clarity to Kantian muddle, but his argument is only optimistically simple where Kant's is justifiably complex.

ON A RECENT ACCOUNT OF ENTAILMENT

MR. P. T. GEACH¹ (following G. H. von Wright's *Logical Studies*, London, 1957, p. 181) is concerned to develop a concept of entailment in terms of which there is something wrong with the following argument-schema :

$$\begin{array}{ll}
 p. \sim p & \text{entails } p \\
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 p & \text{entails } p \vee q \\
 \sim p, p \vee q & \text{entails } q \\
 \therefore p. \sim p & \text{entails } q
 \end{array}$$

I shall call this the first Lewis argument-schema.

Geach says that p entails q if and only if there is an *a priori* way of getting to know that $p \supset q$ which is not a way of getting to know either that $\sim p$ or that q . He expects this to disturb the above argument-schema by falsifying its first premiss, the point being that a truth-table verification of $(p. \sim p) \supset p$ will embody a truth-table verification of $\sim(p. \sim p)$.

But there is an *a priori* way of getting to know (for any particular proposition P) that $(P. \sim P) \supset P$ which is not also a way of getting to know that $\sim(P. \sim P)$; namely, performing a truth-table verification of $(p. q) \supset p$, and substituting P for p and $\sim P$ for q . This would show that $(P. \sim P) \supset P$ without $\sim(P. \sim P)$ occurring as a line in the proof or occurring in a truth-table with a row of 1's or T's in the crucial column—i.e. without our ‘getting to know that’ $\sim(P. \sim P)$ within the meaning of the act.

A parallel argument holds against Geach's objection to what I shall call the second Lewis argument-schema :

$$\begin{array}{ll}
 p & \text{entails } (p. q) \vee (p. \sim q) \\
 (p. q) \vee (p. \sim q) & \text{entails } p. (q \vee \sim q) \\
 p. (q \vee \sim q) & \text{entails } q \vee \sim q \\
 \therefore p & \text{entails } p \vee \sim q
 \end{array}$$

In this, the third premiss appears open to objection from Geach, in that a truth-table verification of $(p. (q \vee \sim q)) \supset (q \vee \sim q)$ will embody a truth-table verification of $q \vee \sim q$, so that this conditional does not correspond to an entailment in Geach's sense of “entails”. But again there is an *a priori* way of showing (for any particular propositions P and Q) that $(P. (Q \vee \sim Q)) \supset (Q \vee \sim Q)$ which is not a way of showing that $Q \vee \sim Q$; namely, showing by truth-tables that $(p. r) \supset r$, and substituting P for p and $Q \vee \sim Q$ for r .

The upshot is that in the Geach-von Wright sense of entailment, any given instance of either of the Lewis argument-schemas holds

¹ “Entailment”, *Proceedings of the Aristotelian Society*, suppl. vol. xxxii, 1958.

relying on this sense of 'necessary'. That anyone should seriously believe this is astonishing. Robinson believes it largely, I think, as a result of his failure to distinguish between Kant's account of 'strict' as opposed to 'unrestricted' generality. I conclude that even Kant's partial or obscure reliance on the fourth sense of 'necessary proposition' is by no means established.

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The upshot is that in the Geach-von Wright sense of entailment, any given instance of either of the Lewis argument-schemas holds

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good. The reasons for putting this (and its justification) in terms of *any* rather than *all*—*i.e.* for talking of argument-schemas and their instances, rather than of arguments each line of which is an implicitly universally quantified proposition have arisen in the following way. During the discussion of the symposium of which Geach's paper formed a part, another of the symposiasts, Dr. C. Lewy, pointed out that von Wright's admission of “*p. q entails p*” as a thesis commits him to an acceptance of the first Lewis argument-schema, just as it stands. Geach replied that von Wright had erred in admitting that *p. q entails p*—that he had probably assumed this to be a thesis because we can prove that $(p. q) \supset p$ with a proof in which neither $\sim(p. q)$ nor *p* is proved on the way; whereas this doesn't really follow, because “*p. q entails p*” is, so to speak, a bundle of instances of itself, while the non-proving of $\sim(p. q)$ in the proof of $(p. q) \supset p$ is the non-proving of a bundle, not a bundle of non-provings, and similarly with the non-proving of *p*. (Explicit use of quantifiers makes the point clear.) The present note steers around this controversy by avoiding any use of (implicitly) quantified entailment-theses, dealing only in (implicitly) quantified theses of truth-functional logic, and introducing mention of entailment only in metalogical remarks about the results achieved (and the manner of their achievement) by the familiar, truth-functional procedures employed. It also makes it clear that von Wright is vulnerable to Lewy's attack not because of a chance remark but because of the central features of his concept of entailment, and thus that Geach is no better off than von Wright in this respect.

Both the Lewis argument-schemas presuppose the transitivity of entailment, and Geach denies his concept this property; but this does not affect the line of argument advanced here. The reason for this is, roughly speaking, that Geach characterises in a certain way the area of non-transitivity of his concept of entailment, and this area does not overlap either of the Lewis argument-schemas.¹ To go into this in full detail would be tedious, especially when the matter can be handled more directly by actually showing in full how we can get to know *a priori* that $P \supset (Q \vee \sim Q)$ is true (for arbitrary *P* and *Q*) by a way in which $(Q \vee \sim Q)$ does not occur as a line in the proof and is not subjected to truth-table analysis:

1. $p \supset ((p. q) \vee (p. \sim q))$ (by truth-tables)
2. $(p \supset ((p. q) \vee (p. r))) \supset (p \supset (q \vee r))$ (by truth-tables)
 - 1 $p/P, q/Q = 3$
 3. $P \supset ((P. Q) \vee (P. \sim Q))$
 - 2 $p/P, q/Q, r/\sim Q = 4$
4. $(P \supset ((P. Q) \vee (P. \sim Q))) \supset (P \supset (Q \vee \sim Q))$
 - 4 = 3 \supset 5
5. $P \supset (Q \vee \sim Q)$

¹ This point was made by Lewy in the discussion mentioned above.

This proof holds for any selection of P and Q ; and a similarly general demonstration can be given of any instance of the other Lewis paradox in a way which also satisfies the Geach-von Wright definition of entailment. I do not give proof here, as (apart from the sort of proof mentioned below) the shortest one I can find runs to fifteen lines, and probably could not be shortened much except at the cost of intolerable complexity of proof-lines and substitution-steps. Anyway, as Dr. Lewy has pointed out to me, either paradox can be derived from the other, in a way satisfying the Geach-von Wright concept of entailment, by contraposition.

JONATHAN BENNETT

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MR. WARNOCK ON ORDINARY LANGUAGE

In his valuable book *English Philosophy since 1900*, Mr. G. J. Warnock, by way of recommending the study of ordinary language as a philosophical method, says "It (*scil.* language) is to be used for a vast number of highly important purposes; and it is at the very least unlikely that it should contain either much more, or much less, than those purposes require. If so, the existence of a number of different ways of speaking is very likely indeed to be an indication that there is a number of different things to be said" (p. 150). And again "Where the topic at issue really is one that does constantly concern most people in some practical way—as for example perception, the ascription of responsibility, or the assessment of human character and conduct—then it is certain that everyday language is as it is for some extremely good reasons; its verbal variety is certain to provide clues to important distinctions" (p. 151). *A priori*, this reasoning looks quite plausible. But there seems to me to be quite a lot of evidence for the opposite conclusion: namely, that differences of forms of expression in ordinary (that is, non-technical) language are not reliably correlated with differences in what is to be expressed; that there is a marked tendency for the same form of words to be used for saying several different things, and for different forms of words to be used for saying the same thing; and that, consequently, the occurrence of a variety of locutions is a poor guide to the variety of facts to be stated or purposes to be served by speech.

Rules of syntax provide many examples of differences in verbal form corresponding to no differences of meaning. Gender, as it occurs in many European languages, is one example. The rule in English prescribing different forms of the verb for different persons and numbers (in the present, but not in the past tense) is another. The criss-crossed functions of "shall" and "will" provide a third.

English, having both a Teutonic and a Romance vocabulary to go at, is particularly rich in groups of words meaning more or less the same thing. If we examine the usage of such synonyms or near-synonyms as "close" and "shut", "mantelpiece" and "chimneypiece", "employees" and "men", "war" and "state of emergency", "defecate" and "shit", "brave", "plucky" "courageous" and "heroic", we find that the choice of one word rather than another is governed by all sorts of considerations besides differences of sense. A word is chosen because it sounds euphonious or snappy or impressive, because of the company we are in (I have heard that there are languages in which the whole vocabulary changes when one addresses inferiors), because it is in the fashion ("lousy", "reactionary", "metaphysical", have all in different contexts been fashionable terms of abuse, replacing more precise characterisations). Where there is a demarcation-rule, it often enough seems to

work on no intelligible principle, as with the rules which forbid us to speak of a herd of sheep or a fox's tail.

The indifference to exact distinctions characteristic of ordinary language is shown up by what happens when a technical word such as "fallacy" or "chronic" or "protagonist" or "complex" (Freudian) is taken up by the vulgar. It is then deprived of its special capacity for marking an important distinction, and made to duplicate the function of some other word or words.

These are random examples contrary to Warnock's thesis. It might be said that they are untypical, and that there are other fields of more philosophical interest in which ordinary language conducts itself more circumspectly. Warnock suggests perception. Let us have a look at it. For describing the visual appearances of things we have in English a large number of expressions : "look like", "look as if", "look as though", "seem like", "seem as if", "seem as though", "seem to be", "appear to be", "appear as if". . . . There are also a number of different things that can be said by means of these expressions. But we do not find that one idiom is specialised for saying one sort of thing; we find that one expression is used to say many different things, and many different expressions are used to say the same thing. "It looks like our cat" can be used (1) to assert a resemblance in visual appearance between another cat and ours, when their numerical distinctness is not in question; (2) to assert that, going by what I can see, I think that a certain object in view is our cat; (3) to assert that, going by what I can see, I should have thought that a given object was our cat if I hadn't had evidence to the contrary; (4) to concede that, in view of what my neighbour has told me, it was probably our cat that stole her chop (here there is no reference to vision at all). In (2) at least, "looks like" can be replaced by any of the other expressions I have mentioned. In a case like (1) we should not normally use "appear"; but we could use "appearance". There is here scarcely a trace of the assignment of particular expressions to particular situations so as to mark important differences. Again, the similarity of "In artificial light, that looks blue" and "In a poor light, that looks new" conceals a very important difference in what is being said; and ordinary language has no clear way of marking this difference.

Or suppose one is investigating the ordinary concept of mind, and its connection with physical concepts. One observes that in "My appointment went clean out of my mind" and "He went clean out of his mind" the same expression is used with utterly different meanings. One observes that in both cases one can replace "mind" by "head", but whereas one can say "out of my head" in the first case, one must say "off his head" in the second. One observes that "He has an excellent mind" is idiomatic English, but "He has an excellent head", in the same sense, is not; yet we say "He has a good head for figures" but not "He has a good mind (or 'brain')

for figures". Here we have a *jumble* of idioms. The likenesses and differences of forms of expression tell us nothing about the likenesses and differences of the facts expressed.

I do not wish to question the received opinion that ordinary language is efficient in its own field, for its own purposes; this is true. But for these purposes vague, rough-and-ready distinctions are usually all that is required. The reason for this is that ordinary language, *ex vi termini*, is concerned overwhelmingly with particular concrete situations, where looseness of language is harmless because the present context prevents ambiguity or unclarity, and an interjection or a shrug of the shoulders can quite adequately express a complex idea. Hence, while ambiguous expressions are as common as blackberries, ambiguous *uses* of expressions are very rare in common speech. For the abstract thinker, dealing with general questions apart from particular contexts, ordinary language is insufficient; not because lawyers or philosophers necessarily deal with matters not dealt with by ordinary language, but because they have to deal with them in general terms with no concrete situation to help them out.

Hence, if you wish to study what ordinary people really think about, say, moral responsibility, you will examine people's actions and attitudes rather than their words. You will consider what persons on what occasions are punished, reproached, shunned, excused, pardoned, given reduced sentences or sent to the doctor. To turn from these actions and attitudes to the, often loose, language in which they are expressed and explained is to turn away from enlightenment. (It might well, for instance, lead you to think that because "diminished responsibility" is a new phrase, it expresses a new concept; this is not the case.)

If, indeed, a philosopher were to show that a particular distinction not generally attended to by other philosophers was provided for in the language, and if he were to show that this provision was found necessary not only in English but also in Tamil and Swahili and Cherokee, then one might expect the discovery to be of philosophical importance. But I do not know of any philosopher who is prepared to go as far as that.

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PRIOR'S 'TIME AFTER TIME', FURTHER CONSIDERATIONS

LET me try to show that it does make sense to ask at what rate time passes and that a moral emerges with the answer. How do we measure time? There are two ways; the earlier is by reference to some phenomenon, e.g. 'So many moons ago', or even, 'so many (clock) hours ago'. The second is sophisticated in all senses; by comparisons with the movement of an indicator on a scale. The former relies on the recurrence of some feature of a phenomenon, the latter does not.

Consider the latter: given a steady velocity it will equal the quotient of distance by time. Let 'v' = the velocity of time, 'u' = the velocity of the indicator, 's' = the distance of a scale division, then

$v = \frac{s}{t}$, where $t = \frac{s}{u}$. Hence, by simple algebra, $v = u$. Thus the velocity of time equals the velocity of the indicator. Since a body in motion is only to be in more than one place in the same time by virtue of extension, and cannot be in more than one place at the same time on any other account, no velocity can exceed the

velocity = $\frac{\text{one unit distance}}{\text{one unit time}}$. This is the velocity of the indicator;

i.e. that of time. From this a condition is imposed on our measurements of time and distance which is expressed by Minkowski thus: 'the essence of this (world) postulate may be clothed mathematically in a very pregnant manner in the mystic formula $3 \cdot 10^5 \text{ km.} = \sqrt{-1} \text{ sec.}$ '. That is, in order to gain consistency the greatest known velocity must be used to act as the indicator, and must be identified with the limiting velocity 1. Thus, from the necessities of consistent measurement the velocity of time equals the velocity of light in this sense; that a measurement of distance is, *aliter*, a measurement of time.

Relativity theory adds to this a definition of *simultaneity*. This supposes that two distantly separated clocks are synchronous when the distant clock would be seen to be slow by the time which corresponds to the distance between them. This at first sight contradicts the first assumption. What is defined is not, however, simultaneity, but the conditions to be accepted for 'mediate congruence'. By the latter expression I mean to convey that comparisons are essentially between juxtaposed bodies, so that when bodies (or clocks) are not congruent some substitute, i.e. 'mediate congruence', has to be defined to allow the comparison.

From all this it follows pretty immediately that time as considered and measured in the framework of consistent measurement has no relation at all to experienced time, or ageing. Further, that the first kind of time measuring can be justified with the second only

indirectly, for measurement describes the orbits of bodies and so on ; one possible behaviour is identified with our sun. Here is the interesting point or moral ; ageing is experienced, so is our sun, these two can be correlated directly : but only as experienced things. Time, as measured sophisticatedly, is wholly irrelevant to this. On the one hand we have the events we perceive and relate, on the other the consistent measurement rules, they won't be brought together. It's no use saying that we have to have experience of the scales and indicators in the second case, for this merely admits straightway that we are not talking of the same thing. The scales tell us about somewhat, there is mediation. We only consider our experience of the scales, the instruments consider the events.

If we limit the word 'know' for a moment to the sense of knowing *systematically*, then we can make the point in this way : what we experience we can't know, what we know we can't experience. I can't see any way for logic, as it stands, to accommodate itself to this, for the present points are merely one example of a more general rule.

JOHN GIBBS

MR. DANTO ON EXPRESSIONS OF THE REFERRING SORT

If a real estate agent were to say to a prospective buyer, "The house on the south-west corner of Riverside Drive and 116th St. was designed by Louis Sullivan", when there is no house at that location, would he be guilty of having uttered a falsehood? Mr. Danto, in a recent note,¹ has argued that "plain men" would regard the agent as having uttered a false sentence, as having lied, "because there was no building to be referred to by the sentence". Danto's argument, of course, is directed against the well-known Strawsonian thesis that, when a sentence which normally has a uniquely referring use, is used in circumstances where there is no definite referendum, "the question of whether it is true or false simply does not arise".² It is Danto's contention that when something is at stake, when there is "something like a moral factor" involved, a question concerning the truth or falsity of such sentences does arise. The plausibility of the Strawsonian thesis, Danto suggests, is due mainly to Strawson's carefully chosen examples, which have been "specially manufactured to serve his polemical purposes". The choice of different examples, presumably, will demonstrate the inadequacy of Strawson's view. Witty and interesting as Danto's remarks are, they do not, I believe, constitute a refutation of the thesis advocated by Strawson. I will try to show the line of defence which is available to him.

When Danto says that plain men would regard the agent's assertions as false, he is not, of course, basing this conclusion upon any statistical survey or factual investigation. He seems to be arguing, rather, that it would be absurd to suggest that any jury of common men would ever refrain from convicting an agent who tried to sell a non-existent house by telling the prospective buyer that it was designed by so-and-so, merely because the sentence the agent uttered had no definite referendum. If Strawson is right, however, a question concerning the truth or falsity of the agent's utterance would never arise, and hence the agent would not be construed as having uttered a falsehood. How then could we have any grounds for legal action against the unscrupulous agent? Clearly, we would hold the agent to be legally culpable, and from this it follows, Danto seems to think, that the sentence uttered by the agent would be regarded by the jury as false.

There is no doubt concerning the truth of Danto's main premiss. It is highly unlikely that any jury would ever fail to convict an unscrupulous agent merely because he used a uniquely referring expression which failed to refer because in the context of his utterance there was nothing to be referred to. It does not follow, however,

¹ Arthur Danto, "A Note on Expressions of the Referring Sort", MIND, July 1958.

² P. F. Strawson, "On Referring", MIND, July 1950, p. 330.

that what the agent said was false, and hence, it does not follow that Strawson's thesis is defective or unable to account for such cases.

Danto is led to make an unwarranted inference, I believe, through a failure to take seriously Strawson's distinction between the statement which is made and what is 'implied' (in a special sense) by the making of that statement. In *saying* that the house on the south-west corner was by Sullivan, the agent '*implied*' that he believed that there was a house on the south-west corner. At least in the primary use of expressions which are used to refer or mention, people do not usually talk about something unless they believe there is something to be talked about. Thus, if the prospective buyer had no reason to distrust the agent (or at least no reason to distrust everything the agent said), he would naturally be led to believe that there was such a house, and he would be led to believe this because the *utterance* of an expression which is normally used to refer 'implies' that the utterer believes or wishes to be taken to believe that there is a definite referendum for the expression. Nevertheless, the agent did not *say* that there was a house at that location, nor does the sentence, "The house on the south-west corner is by Sullivan" logically imply that there is such a house. The sentence 'implies' there is such a house only in what Strawson calls "a very special and odd sense of 'imply'", a sense of the word which must be distinguished from logical implication.

The agent who said "The house on the south-west corner is by Sullivan" was intentionally (we may assume) using an expression which normally 'implies' that he believes there is such a house. He was using language with the intention of deceiving the prospective buyer, i.e. attempting to mislead the buyer into buying a house which doesn't exist. But it does not follow from the fact that the agent was using language to deceive that everything he says must be *false*. In particular, it does not follow that the sentence uttered by the agent must be *false*, even though the agent uttered it while attempting to convey a false impression. There are ample grounds for legal action against the unscrupulous agent, even if what he said is not construed as being *false*.¹ By saying what he did, he led the buyer to believe something that was *false*, namely, that there was a house on the south-west corner. So, no absurdity results from applying Strawson's thesis to this particular case, or, indeed, to any case where a moral factor is involved.

Danto argues further that the sentence, (i) "But there is no house on the south-west corner", "*exactly contradicts*" the sentence, (ii) "The house on the south-west corner is by Sullivan". "How else could we contradict it?" he asks.² There is an ambiguity, however, in the word 'contradict', and when the various senses of the word are distinguished, it becomes clear, I believe, that (i) contradicts (ii) only in a broad and philosophically uninteresting sense.

¹ Cf. W. L. Prosser, *Handbook of the Law of Torts*, chap. 18, *passim*.

² Danto, *ibid.* p. 406.

In a logical sense, the proper contradictory of (ii) would be (iii)
 "The house on the south-west corner is *not* by Sullivan". On the other hand, there is a common use of the word 'contradict' such that a sentence may be said to contradict another any time the former is used to reject or oppose the latter. In this sense, (i) does contradict (ii), for in saying that there is no house on the south-west corner, one is opposing (disagreeing with) the assertion that the house was designed by Sullivan. It would be a mistake, however, to infer from this that both (i) and (ii) must have truth-values and that those truth-values must differ. To make such an inference would be to confuse the precise logical sense of the word with the looser ordinary sense.

When a person uses an expression which is normally used to refer uniquely in a context where it fails to refer, we often reject or oppose his assertion by uttering a sentence which logically contradicts the sentence 'implied' (in the sense discussed above) by the sentence which is uttered. Thus, when the agent said (ii) "The house on the south-west corner is by Sullivan", the buyer might have replied by uttering (i) "But there is no house on the south-west corner", which is the denial of (iv) "There is a house on the south-west corner", a sentence which was never uttered but which was 'implied' by the utterance of (ii). In replying in this fashion to the agent's utterance, the buyer would have been giving a reason for saying that the question of whether (ii) is true or false simply does not arise.

If the line of defence suggested above is sound, as I believe it is, then Strawson's thesis is clearly immune from the kinds of objections forwarded by Danto. Strawson can consistently account for cases where referring expressions fail to refer, even when the utterance of such expressions has important legal or moral consequences.

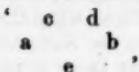
ROBERT AMMERMAN

University of Wisconsin

MR. COPI ON OBJECTS, PROPERTIES AND RELATIONS IN THE *TRACTATUS*

I AGREE with some of what Mr. Copi says. But he has no justification for asserting that according to the *Tractatus* 'the fact that aRb contains exactly two elements'—namely the objects a and b. All that the *Tractatus* tells him is that the objects a and b occur 'in the sense' of the proposition 'aRb' (4.1211). 50 or 1000 or an infinity of other objects may occur in that sense as well.

Accepting what I think is Mr. Copi's suggestion—which coincides with my own view—that Wittgenstein's theory demands that there shall be as many different possible expressive relations between 'a' and 'b' as there are different possible relations between a and b, the following arrangement of names might be one of them :



and this might be the fully analysed form of 'aRb'. There is no difficulty, as Mr. Copi seems to think, about objects' having external properties. For example, a possible definite description of an object, e.g. 'R'b'—'the (thing that is) R to b' would give possible external properties of it (*cf.* 4.023); of course it would not define the object. Or again, if aRb, 'Rb' gives an external property of a.

G. E. M. ANSCOMBE

University of Oxford

VIII.—NEW BOOKS

Observation and Interpretation. A Symposium of Philosophers and Physicists. Edited by S. KÖRNER, in collaboration with M. H. L. PRYCE. London, Butterworth's Scientific Publications, 1957. 40s. net.

THE two main topics discussed by the symposiasts are probability and quantum mechanics. Is probability a logical relation, a propensity, a betting rate, objective or subjective? Ayer argues against the logical theory. If the probability of a hypothesis depends logically on the evidence, then—according to Ayer—we cannot justify the principle of total evidence; it implies that the same hypothesis may be more or less probable with changing evidence; and this makes it impossible to take probability as the guide of life. Braithwaite says that the progress of science consists in betting successfully with nature. Popper proposes the propensity interpretation of probability. It is a version of the frequency theory: probability is taken as a physical property of actual events, though the experimental arrangements under which they take place are included here. The theory is said to be 'objective', purely statistical though 'reformed', and to eliminate the 'subjective' interpretation in terms of incomplete knowledge.

This brief summary shows, I think, that the issues raised, however interestingly formulated, are of old standing within the discussions of probability and that the philosophical problems involved have not been deeply touched. Ayer's argument is not very strong. There exists a calculus of probability. If probability is a mathematical concept, and if mathematical statements are logically true, how can we escape the conclusion that a statement of probability is logically true? There is no need to justify the principle of total evidence beyond saying that the more evidence we have, the more we know. After all, probability is a measure of knowledge, not of ignorance (as is often alleged) which is immeasurable. That the probability of a hypothesis changes with the evidence and, therefore, sometimes with the speaker is then obvious. And why should probability be the guide of life? Only because Bishop Butler said so? Surely, this can mean no more than that mathematics, or logic, or reason, *tout court*, is used by us for calculating what is calculable in our lives.

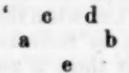
To say that science is a game we play with nature, at once raises the question, What are the rules? A game must be played according to the rules established for it, and both opponents have to follow them. What happens if we do not know the rules, or if we do not know them completely? And does nature react to our moves like the opponent in a game? No doubt, the model of a *specific* game, e.g. dicing, is sometimes useful for representing a particular situation we want to investigate. But to generalise this and to assume a fixed betting rule immediately suggests that nature follows it, that is, we introduce inductivism.

And what does it mean to assert, as Popper does, that probability is subjective when our knowledge is incomplete? We would not make use of the concept of probability unless we feel that there is more to know. This attempt at resolving a dichotomy (which is anyway a false one) smuggles in the ideal of Perfect Knowledge that represents the standard of objectivity. Does this make sense? All these difficulties are artificially created when probability is taken as an *ex ipso* prescriptive rather

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And what does it mean to assert, as Popper does, that probability is subjective when our knowledge is incomplete? We would not make use of the concept of probability unless we feel that there is more to know. This attempt at resolving a dichotomy (which is anyway a false one) smuggles in the ideal of Perfect Knowledge that represents the standard of objectivity. Does this make sense? All these difficulties are artificially created when probability is taken as an *ex ipso* prescriptive rather

than a descriptive concept. In this way, to follow probability becomes a moral maxim, i.e. the guide of life. Moral rules must be justified, so we have to give convincing reasons why we choose the total evidence, etc. Thus, nature becomes another human being, a partner who is so moral that he cannot cheat. If we realise that probability, like any other concept of mathematics, is descriptive, then no such difficulties arise. What evidence we choose, and how much of it we consider in a given instance, is then decided by criteria that lie outside probability theory, namely, by factual and theoretical knowledge of the field in which we apply it.

The discussion on quantum mechanics is, I think, more successful. In recent years Bohm has tried to put determinism into quantum physics. His first attempt at introducing 'hidden' variables into the theory for this purpose was too simple and useless. At this conference he takes a more subtle approach : determinism is to come back only on the sub-quantum level of the elementary particles. It is at least *prima facie* possible that a new theory, unlike quantum mechanics, may turn out to be deterministic. The only trouble is that no such theory exists as yet ; and Bohm admits that his work has not yet yielded any results. His idea is that from a sub-quantum theory the ordinary quantum mechanics should be derivable as Newtonian mechanics can be derived from quantum mechanics. There is, I think, a difference. We know how to derive a deterministic from a statistical theory, namely, by averaging over certain variables. If Bohm's proposal is right, we must also find a procedure that can achieve the opposite, that is, derive a statistical theory from a deterministic one. A statistical theory is richer than a deterministic theory, for it contains more variables : how could it be derived from a poorer theory without introducing extra assumptions ? We do not even speak of deriving kinetic theory from Newtonian mechanics, and yet we need a special assumption, i.e. the ergodic hypothesis, to relate one to the other. Classical determinism and the statistical laws of quantum physics are regarded by Bohm as extreme, idealized, limits ; any actual theory contains laws showing traits of both. This is at least an interesting speculation, though it is difficult to assess exactly what it means.

After more than a quarter of a century of discussion, indeterminism is still felt to be objectionable. Why ? Both deterministic and statistical laws are represented by the same kind of mathematics. It may be that a new theory for the sub-quantum level turns out to be deterministic like Newtonian mechanics. It is highly unlikely, since there we have to deal with mass phenomena as in quantum mechanics, and statistics is the proper representation for such phenomena. The false opposition of causal and statistical law has so often been exposed that emotional rather than logical or physical meaning seems to be involved if the discussion is revived today.

In contrast to Bohm, Rosenfeld rejects any heterodoxy regarding quantum mechanics and upholds the 'Copenhagen' interpretation. I agree with him most heartily when he condemns 'the outdated materialistic metaphysics of the nineteenth century' which lies at the bottom of the anti-orthodox criticism. But isn't it too narrow to say that there is no room for an arbitrary element in science and optimistic to claim that the road to future progress is clearly marked ?

The other papers relating to quantum mechanics, especially those on the theory of measurement, are so overwhelmingly—and quite unnecessarily—expressed in mathematical symbols that their philosophical import, if

any, is totally lost. Symbolism is useful if it helps to make clear what is talked about : it blinds when it is no more than a display of mathematical fireworks.

Finally, the purely philosophical papers contributed to the symposium somehow bring up the rear. Körner argues that 'some metaphysical propositions are, or function as, regulative principles'. Polanyi points out how important aesthetic considerations are when we construct a theory ; he relates them to the reality the theory describes and to the satisfaction we feel in accepting it. This is, I think, quite an important criterion which scientists in fact appeal to, while methodologists have so far overlooked it. In contrast, Körner's argument has often been put before, and though there is something in it we must also cry caution here. It may well be that the metaphysics of the classical philosophers was meant to be physics of a speculative sort providing, as it were, a model of the universe or a conceptual framework within which a scientific theory is formulated. Physics was then not sufficiently developed and so there was more scope for fantasy. Today, when physics, and science in general, has so much advanced our knowledge of reality, the return to metaphysics—however fashionable at the moment—is sheer regression. Without denying that there are always metaphysical statements in science (in the historical sense of this term), we progress only by eliminating them ; physics provides many examples, *viz.* the aether problem. The modern view of metaphysics as second-order statements helps in its rehabilitation ; even then, our intent in using such statements makes metaphysics quite different from what it was in the past.

Kneale surveys the various meanings of the word 'see' and argues in favour of the sense-datum theory of perception. This is a philosophical theory that has had its uses, but I would disagree with Kneale's judgment that it is harmless. It seems to me to be just as misleading to analyse perceptual statements in terms of visual sensa as it is to speak always of seeing material objects. The sense-data language is no more basic than the thing-language.

Gallie and Ryle discuss prediction—unfortunately, both papers are very brief. What do we mean by 'prediction', and are there any unpredictable events ? This brings us back to the topics of probability and determinism with which the symposium begins. Gallie points out that to be unable to predict an event tells us less about it than about the limitations of our knowledge. The meaning of 'prediction' and of 'determinism' are interrelated, and so we must examine more closely the grounds on which we judge a system to be determinist. I would stress here that no absolute meaning can be ascribed to terms like randomness or determinism—events are random only with respect to known regularities, *i.e.* a determinist law.

Not all predictions are inferences ; and those that are do not make assertions about the future exclusively, but often only about the unobserved present. This is, according to Ryle, important to notice if we want to understand induction. I would agree that neither to forecast nor even to predict is the principal job of the scientist. This 'crystal ball' view of science is a hallowed delusion expressed, for example, in the inductive conception of probability, namely, that it is the guide of life. An experimental inference may have a prediction as the conclusion ; and such an inference has always the purpose of testing a hypothesis. A predictive inference may be said to be successful only if at least three conditions are fulfilled : the conclusion must turn out to be true ; it must have been

derived from true (observational) premisses ; and the derivation must follow accepted rules. Indeed, the criterion of success is too naively interpreted by philosophers and scientists alike. To be satisfied with saying, It works, is not enough for science when we know that faith can move mountains. I would say that to make the criterion clearer is the most pressing task for the philosopher of science who wants to eliminate metaphysics—like determinism and inductivism—from physics or to separate science from prophecy.

E. H. HUTTEN

The Unconscious. By A. C. MACINTYRE. London : Routledge & Kegan Paul, 1958. (Studies in Philosophical Psychology.) Pp. ix + 100. 12s. 6d.

The Concept of Motivation. By R. S. PETERS. London : Routledge & Kegan Paul, 1958. (Studies in Philosophical Psychology.) Pp. 166. 14s.

In spite both of their common membership of the new 'Studies in Philosophical Psychology' series, and also of the similarity of subject-matter with which they are concerned, it seems best to consider these volumes separately. It is difficult to believe that they will find places on the same book-shelves.

In *The Unconscious—a conceptual study*, Mr. MacIntyre presents an orthodox account of the logic of some of the concepts employed by psychoanalysts. It is the principal contention of the book that many of the analytic concepts, such as those of unconscious wish or unconscious anxiety, are derived from simple and limited alterations to the use of 'wish' and 'anxiety' in ordinary speech. This close relation with the common meanings which are attached to such terms is, he rightly points out, a necessary source of strength both in analytic theory and also in actual clinical practice. The view is developed further : "Understanding how a purpose might be unconscious is a necessary step in understanding in anything but a fragmentary way what we mean by 'purpose' at all." Mr. MacIntyre therefore espouses Freud's own classification of himself with the "imaginative describers" of human conduct. The author recognises that this kind of analysis of the meaning of psychoanalytic statements is not new. But this is the first occasion, so far as I am aware, that this thesis has been developed in any detail. The argument is clear and based upon a good, though brief, exposition of Freud's findings and theory.

It is a necessary corollary of this main contention that Freud was, to a great extent, providing something other than causal explanations of behaviour. This seems to be substantially correct, though Freud himself sometimes accepted this view and sometimes insisted upon the causal nature of his theories. In arguing this point, however, Mr. MacIntyre claims that psychoanalysts in fact tend to give illuminating *descriptions* of behaviour, rather than causal explanations. This argument is presented in the fourth chapter of the essay entitled 'Describing and Explaining', and may be regarded as the core of the book. Now it is not appropriate here to discuss the proper use of 'describe'. And, in so far as all that is required is a general expression with which to refer to accounts of behaviour of a non-causal nature, no great harm is done to the central argument of

the book by this very general use of the word. On the other hand, to claim that when a psychoanalyst says that a person suffers from some phobia because he has an unconscious wish of some kind, he is only *describing* the person's behaviour, is somewhat misleading; for, surely, not all explanation is causal explanation. At least part of the interest of psychoanalytic statements is that they do purport to explain instances of behaviour which may be more or less adequately described in the absence of such explanations. This is not to deny that much description does involve interpretation and, at least implicitly, explanation. But, to take a case which is discussed at some length in this part of the book, to speculate about someone's intentions is not to *describe* his behaviour.

Mr. MacIntyre rightly emphasises the purposive aspect of 'intention', together with that of such terms as 'wish', 'motive', etc. But he seems to think that reference to goals *describes* behaviour, whereas, in general and granting exceptions, I should have thought such reference explained behaviour which could be described independently. This part of the argument, in so far as it attempts to give a positive account of the meaning both of ordinary and of psychoanalytic terms of this group, in addition to distinguishing the group as a whole from causal explanations, seems to be the weakest part of the essay. And it is perhaps partly due to the fact that something more than descriptions are given by such statements that led analysts and others to suppose that causal explanations had been provided.

Even if it is accepted that much of Freud's insight was into the motives for, rather than the causes of, behaviour, it is clear that his thinking was greatly influenced by his earlier scientific work and that his analysis of the motives of behaviour is often presented as though it were a causal account. Mr. MacIntyre recognises this and traces the influence of neurological ideas upon Freud's theoretical work. However, while he is of the opinion that, for the most part, the value of psychoanalytic ideas has been to describe (or explain) behaviour in non-causal ways, he does seem to be convinced that there remains a task of causal explanation to be accomplished, namely to explain in causal terms the correlations, to which Freud drew attention but failed to explain causally. These include such examples as the relation between potty-training and later character traits; and, at the end of the essay, some of the difficulties are discussed which hinder the conduct in this field of the experiments which are necessary to obtain the evidence required to develop any such theory. But it is far from clear what are the grounds for this conviction in the validity of these alleged correlations. Certainly causal explanations are needed, and it may be that psychoanalytic theory has in fact suggested at least some of them. But the evidence for this claim is far from self-evident. And, of course, even if such correlations were established, this alone would not show that these particular childhood experiences were effective causally in producing the symptoms in question.

It has seemed worth-while to raise these points since, although they are not entirely central to the theme presented, the main arguments and conclusions of the essay are important in that they bear some implications for clinical and experimental practice. That this is so is partly due to the fact that, although he is engaged upon a strictly conceptual analysis, Mr. MacIntyre has taken care to understand, and to have constant reference to, the ideas and therapeutic procedures derived from Freud's work. It is precisely in this that the second of these volumes, *The concept of Motivation*, fails so sadly.

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the book by this very general use of the word. On the other hand, to claim that when a psychoanalyst says that a person suffers from some phobia because he has an unconscious wish of some kind, he is only *describing* the person's behaviour, is somewhat misleading ; for, surely, not all explanation is causal explanation. At least part of the interest of psychoanalytic statements is that they do purport to explain instances of behaviour which may be more or less adequately described in the absence of such explanations. This is not to deny that much description does involve interpretation and, at least implicitly, explanation. But, to take a case which is discussed at some length in this part of the book, to speculate about someone's intentions is not to *describe* his behaviour.

Mr. MacIntyre rightly emphasises the purposive aspect of 'intention', together with that of such terms as 'wish', 'motive', etc. But he seems to think that reference to goals *describes* behaviour, whereas, in general and granting exceptions, I should have thought such reference explained behaviour which could be described independently. This part of the argument, in so far as it attempts to give a positive account of the meaning both of ordinary and of psychoanalytic terms of this group, in addition to distinguishing the group as a whole from causal explanations, seems to be the weakest part of the essay. And it is perhaps partly due to the fact that something more than descriptions are given by such statements that led analysts and others to suppose that causal explanations had been provided.

Even if it is accepted that much of Freud's insight was into the motives for, rather than the causes of, behaviour, it is clear that his thinking was greatly influenced by his earlier scientific work and that his analysis of the motives of behaviour is often presented as though it were a causal account. Mr. MacIntyre recognises this and traces the influence of neurological ideas upon Freud's theoretical work. However, while he is of the opinion that, for the most part, the value of psychoanalytic ideas has been to describe (or explain) behaviour in non-causal ways, he does seem to be convinced that there remains a task of causal explanation to be accomplished, namely to explain in causal terms the correlations, to which Freud drew attention but failed to explain causally. These include such examples as the relation between pottng-training and later character traits ; and, at the end of the essay, some of the difficulties are discussed which hinder the conduct in this field of the experiments which are necessary to obtain the evidence required to develop any such theory. But it is far from clear what are the grounds for this conviction in the validity of these alleged correlations. Certainly causal explanations are needed, and it may be that psychoanalytic theory has in fact suggested at least some of them. But the evidence for this claim is far from self-evident. And, of course, even if such correlations were established, this alone would not show that these particular childhood experiences were effective causally in producing the symptoms in question.

It has seemed worth-while to raise these points since, although they are not entirely central to the theme presented, the main arguments and conclusions of the essay are important in that they bear some implications for clinical and experimental practice. That this is so is partly due to the fact that, although he is engaged upon a strictly conceptual analysis, Mr. MacIntyre has taken care to understand, and to have constant reference to, the ideas and therapeutic procedures derived from Freud's work. It is precisely in this that the second of these volumes, *The concept of Motivation*, fails so sadly.

To deal faithfully with all the misunderstandings in this book would require a work several times the size of the original. It is more than doubtful whether any good purpose would be served by such an endeavour. Instead, I shall attempt only to characterise the argument in general, and to discuss very briefly one or two of its central ideas. It is Mr. Peters's argument that psychologists, of all varieties, have been concerned to devise what he calls 'over-all theories of motivation'. The two instances which he considers at length are Freud's theory and 'Drive' theories. Such theories, he claims, are logically impossible and hence psychologists have wasted much time and effort in conceptual confusion. A number of logical considerations are advanced in support of this argument and to the relevance of these I will return. First, one or two preliminary objections should be made. Since Freud's ideas have been the subject of discussion above, I shall concentrate on that part of the book concerned with explanations of behaviour in terms of drives.

It is always surprising when considerable progress is made towards a logically impossible end. The author, faced with the rapidly increasing body of experimental evidence which, favourable or unfavourable to the 'drive' hypothesis, in large part stems from predictions from this hypothesis, feels called upon, towards the end of the book, to comment on this phenomenon. It is with some disappointment that one finds that the best that can be done is to dismiss this research by attributing it to the need to 'publish or perish' . . . (p. 154). This raises the suspicion that the author is not really in touch with his subject-matter; and this is fully confirmed elsewhere in the essay. We are told, for instance, that we know so much about human behaviour that all that is required is to make it explicit (by the analysis of the concepts of ordinary language). We need not attempt to obtain more evidence. Again, in abnormal psychology, in which facts are perhaps more sparsely scattered and less well substantiated than in any other branch of psychology, it is claimed that plenty of facts are known and that all that is required is to devise an adequate conceptual framework into which to fit them. These, and similar comments, are distressing; but when it is confidently asserted that, in experimental work with animals, questions concerning their motives do not arise and that, for instance, the question whether the reason for a piece of behaviour was curiosity or some other motive could, at best, only arise occasionally in connection with the behaviour of primates, one cannot escape the impression that Mr. Peters has never seen the behaviour in question. It is at least plain that Mr. Peters has never attempted, in no matter how schematic a form, to outline any mechanism for goal-directed behaviour. Had he, at any time, indulged in such speculation as an exercise, he would not have found Tolman's 'confession of faith', that there was an essential identity between the behaviour of rats in mazes and human behaviour, social and linguistic behaviour excepted, so 'empty'; although he might very well have been led to the conclusion that Tolman was wrong.

But, apart from suggesting that this essay fails because it is so much out of touch with the facts to be explained and the nature of the problems arising in attempting to explain them, one or two central points of the argument must be mentioned. Some of the reasons given for the conclusion that 'drive' theories are logically inappropriate are as follows: (1) Causal explanations can only be given for phenomena which can be described objectively and in neutral terms: movements, but not actions, such as signing a cheque, can be so described. But behaviour is composed of actions, not movements. Hence causal explanation is impossible. (2)

The notion of 'drives' implies that people are 'driven to' do things. This is sometimes, but not usually, the case. Obsessional neurotics are 'driven to' wash their hands, but not everyone's hand-washing activities are of this kind. Hence, to apply 'drive explanations' to other than abnormal cases confuses an important distinction and is logically incorrect. (3) 'Drive' explanations are of a 'mechanical' variety. But 'mechanical' behaviour is to be contrasted with ordinary "rule-following purposive behaviour," and hence mechanical, causal explanations are not appropriate for the latter. They are appropriate to explain what happens during 'conditioning' but not what happens during 'training'. (4) Such explanations are based mainly upon the study of the behaviour of rats. There is no reason to accept, and there is some empirical evidence in conflict with, the supposition that all human behaviour may be explained in this way. (5) The specification of 'drive' states and mechanisms could never be more than an account of some of the necessary conditions for behaviour. It could never give sufficient conditions, and hence no adequate explanation can be provided in this way.

These are some of the principal objections which lead Mr. Peters to reject, as logically inappropriate, explanations of ordinary behaviour in terms of drives. None of them seem to be good reasons for doing so. For, (1) it is not at all clear why causal explanations cannot be given of actions, as opposed to movements. This has, in fact, been done in many cases. It may, of course, be easier to explain an instance of behaviour causally, when exactly the same movements are employed upon each occasion that it is executed. But even in this case, reference will usually have to be made to the goal-setting and goal-recording mechanisms which are involved in the control of the behaviour. If the ways in which an action is performed vary from time to time, this may require more complex mechanisms of motor control; but it is difficult to see that any *logical* difficulty is introduced by this requirement. And are we to say that causal explanations cannot be given of the behaviour of the many types of goal-seeking machine which now exist? This would seem to involve a very curious meaning of 'causal explanation'. (2) To infer from the ordinary use of 'driven to' to the technical use of 'drive', and to conclude that, since the former applies only to special cases, so also must the latter, seems little more than trivial. The term 'drive' has been used to denote some of the control mechanisms which select the goals which organisms seek, which record the fact that they have been found, and so on. Even among theorists who accept a 'drive' variety of explanation of the details of these mechanisms, in a given case there may be much disagreement. And, of course, it is debatable which, if any, aspects of behaviour are controlled by mechanisms which are of the kind which have been termed drives. But these are empirical disagreements; the ordinary use of 'driven to' (Mr. Peters only once refers, implicitly, to the ordinary use of 'drive') has simply nothing to do with the matter. (3) It is difficult to see what Mr. Peters has in mind in the distinction between 'mechanical' and other types of behaviour. If the point is that some behaviour is stereotyped, while other instances of behaviour are variable, the importance of the point seems to be slight. For it has long been recognised that such variations in the observable characteristics of behaviour provide no very reliable guide to the nature of the mechanisms controlling the behaviour. If, however, the point is that only some behaviour such as that found in conditioning can, in principle, be *explained* in terms of causal mechanisms, while other aspects of behaviour, such as the modifications occurring during 'training' cannot be

so explained, then the claim seems to be quite false. For there are several 'mechanisms', the causal properties of which are well understood, which imitate 'training' rather than 'conditioning'. It seems likely here that the author has far too simple-minded an idea of the *kind* of mechanisms which are relevant. (4) The extrapolation of the results of research on motivation in animals is commonly regarded with extreme caution by psychologists. This is not to say that no attempt is made to find out how far such explanations could carry us. This is worth-while if only because it brings out the limitations of certain kinds of mechanisms. But to suppose that psychologists in general are in the habit of simply assuming that 'drive' theories of motivation, or any other theory, will automatically apply to all behaviour of all species is indeed to live in a world of hypothetical constructs. Hull, for instance, certainly believed that his account of drives was substantially applicable to a large part of the behaviour of rats; and he hoped that it *might* have some direct application to human behaviour. He intended to see how far he could take it. But even if no aspect of work with animals could be extrapolated to human behaviour, the reasons for pursuing such research would be largely unaffected. For, apart from the intrinsic interest of these findings, it is reasonable to suppose that until we are in a position to understand comparatively simple mechanisms there is little chance of our understanding the more complex human case. In supposing that the purpose of such research is to provide grounds for extrapolation to the explanation of human behaviour, Mr. Peters seems largely to have missed the point. And, in any case, how far such an extrapolation is justified is not a logical but an empirical issue to which no definite answer can as yet be given. The empirical grounds which are given for denying the adequacy of 'drive theories' for human behaviour are drawn mostly from experimental work on animals which, it is claimed, suggest that even here no comprehensive 'drive' theory could be adequate. As a matter of fact, the evidence quoted does not, for the most part, bear against such theories; there is other evidence, which is not mentioned, which does bear against them. (5) The significance of the last claim is difficult to assess. For of course psychologists have not supposed that the specification of 'drive' mechanisms, even in the cases of behaviour to which they are thought to be applicable, provides a *sufficient* explanation of behaviour. I find it impossible to see how anyone could think that this was so. It is obvious that there must be mechanisms involved in memory, in sensory analysis and motor control and a host of other functions. These functions are entirely distinct and in addition to those served by motivational mechanisms. Then, in order to explain a given piece of behaviour, variable external and internal conditions would have to be specified. The point of arguing, therefore, that 'drive' theories of motivation, or Freud's theory, or any other, can at most provide only necessary conditions for behaviour, escapes me.

I have discussed these points in detail because it seems to me that this book will present, to those who have no knowledge of psychology, a thoroughly misleading picture of the kinds of problems with which the subject is concerned and where the conceptual confusions, which are common enough in the subject, are to be found. Any psychologist who reads the book will find himself, at the end, wondering what all the fuss was about.

A. WATSON

An Essay on the Foundations of our Knowledge. By ANTOINE AUGUSTIN COURNOT (translated by Professor M. H. MOORE). The Liberal Arts Press, New York, 1956. Pp. lxviii + 615.

COURNOT was born in 1801 and died in 1877. His contributions to statistical theory especially in its application to economics are fairly well known, but his contributions to philosophy seem to have been ignored in a way which they certainly do not deserve. By translating his treatise "On The Foundations of Our Knowledge", published in 1851, Professor Moore has attempted to rectify this unfortunate state of affairs.

According to Cournot the philosopher's aim is to discover the order and reason of things. This is also the scientist's aim; he seeks, for instance, to discover the order of the material world and the various ways in which human relationships are ordered according to moral precepts, economic necessities, and so on. Because of his concern with order and arrangement and therefore with abstract truth, the attitude or spirit of the scientist has a good deal in common with that of the philosopher. Nevertheless science is no substitute for philosophy nor *vice versa*; despite their intimate relationship and some resemblances the two disciplines are quite distinct and should never be confused. Not only do they advance and develop differently but philosophical ideas do not fall within the field of sensible experience. Moreover, the characteristic of philosophical truth is that it can neither be put beyond the possibility of dispute nor be categorically demonstrated. In philosophy there is a feeling for, or sense of, what is true, rather like the feeling for what is beautiful in the arts, but the upsetting of common sense with which philosophers are so often charged, like the perversion of good taste, is not, properly speaking, an error which amounts to a refutation. Whereas the scientist studies both man and nature, the subject and the object of knowledge independently, philosophical speculation is never at liberty to deal with them separately. From this spring both its distinguishing characteristics and special difficulties.

To judge that in certain respects our ideas conform to the reality of things is to affirm that the true relations of things are not falsified or complicated by our perceptions. This is not to pretend that it is within our power to attain absolute truth. The function of philosophical criticism is, Cournot thinks, to put us in a position to make judgements about the representative value of our ideas. To attain such a position however, neither requires nor presupposes empirical or psychological research. We do not have to know how our ideas come to be formed, nor what phases our impressions and perceptions passed through before taking the definite forms that the laws of our nature assign them. If we consider our sensations, it is not difficult to distinguish those which have a representative value from those which do not represent anything, at least not directly, although they may inform us of the presence of external objects that are depicted and so known by other sensations. Consequently some species of sensations could not be suppressed without destroying or upsetting the system of our knowledge, while others may be abolished without giving rise to any necessary alteration or mutilation of our knowledge, whether common or scientific. An analogous distinction holds in the field of abstract or general ideas. Some have a basis only in the constitution of our mind, in the nature of its instruments, and in the needs of our methods. Others represent the principles of unity and harmony that nature has put into its works. These latter are principles which we could not invent but which we must grasp and express in the best way we can, although usually with

imperfections of which we are aware and which we strive ceaselessly to remedy. In so far as philosophical criticism enables us to discover principles of this latter kind it has a positive side. The principles of philosophical criticism itself are the same as those of every type of criticism.

Now there is a very important disparity between the things to be represented and the means of representing them. The reason for this, so Cournot thinks, is that human understanding is so constituted that it can have direct representation or intuition only of extension or of the forms of space. For everything else it has to rely on symbols and language or on the symbolic use of the forms of space as auxiliary means of indirect representation and expression. In other words human understanding has to rely on defective instruments. For whereas nature generally follows the law of continuity in all things in such a way that discontinuity appears only as a peculiar or accidental case, conventional symbols such as those of language, lend themselves only to the expression of clearly distinct ideas and relations, among which there are no continuous transitions or indiscernible nuances. As for spatial figures, these lend themselves fully to the representation of continuous things only in the very special case in which their continuity is associated with measurable magnitude. But this is not all, another important source of the disparity in question is due to the linear form of discourse. Our study of things and of the relations between them confronts us with an infinite variety of ways in which they are ordered, but the linear form of discourse does not allow us to make this variety perceptible or to fix it except by means of imperfect images borrowed from the figures of geometry.

From all this it follows that logic, which draws its name and form from the name and form of language, is often a rebellious and natively defective instrument, as much for the perception as for the explanation of the true relations of arrangement between things. The insufficiency of logic has often been felt and proclaimed by catching it in contradiction with the indications of good judgement or reason. The objects of philosophical speculation can, for the most part, be expressed only imperfectly or approximately by rigorous definitions of ideas and logical connexions of propositions. The task of grasping and expressing the intelligible relations of things will therefore for the most part no longer be the work of the calculator who advances by sure and reckoned steps, applying methods, combining or developing formulae, and connecting propositions. It will be the work of an artist. In his own way the philosopher will be a poet or a painter; this is why a mask of personal individuality is stamped on the productions of his mind, and explains why philosophical and scientific speculation do not have a parallel development.

But what the philosopher believes about the order and reason of things is never beyond possible dispute and can never be categorically demonstrated because of the deficiencies of his linguistic instruments; if the philosopher is in many ways like a poet or painter and depends on his feeling for what is true, as Cournot has said; what sort of weight have the philosopher's assertions, and what kind of arguments does he use to support them? Cournot maintains that philosophical speculations can quite properly be regarded as more or less probably true. He distinguishes between two kinds of probability, 'philosophical' and 'mathematical' respectively. The source of the former is our feeling for the order and reason of things and this probability is the basis of most of the judgements we hold in speculations of the higher sort as well as in the most ordinary affairs of life. Philosophical and mathematical probability have certain

resemblances. Both are capable of being increased or decreased imperceptibly ; and each is connected, although in different ways with the notion of chance or the independence of causes. They also differ in various ways, the most important of which is that philosophical probability is always incompatible with the possibility of numerical evaluation. It is also subjective in the sense that it does not always have the same force for different people. Although Cournot distinguishes between science and philosophy and is quite clear that to attempt to verify philosophical speculations by empirical observations and experiment is absurd, he seems, nevertheless, to think that there is some affinity between philosophical speculations and some scientific explanations. He points out that many scientific explanations have philosophical probability and that it would be silly to regard them as having probability of the mathematical kind. I take it that in so far as we are concerned with the order and reason of things when doing philosophy, Cournot assumed that we were dealing with a question of fact which has to do with the constitution of the world. He therefore thought it reasonable to speak of the probability of the conclusions we reach. I venture this as an explanation of what he says but not as a justification of it.

Assuming that Cournot's own procedure is an indication of how he thought the philosopher should go about his business, then the philosopher's main task is to get his readers to understand as clearly as possible what he has to say. This is accomplished by distinguishing carefully between the different senses of the key terms under discussion and the liberal use of examples, and for the rest depending on the good sense of the reader. Nor are the examples restricted to one special field but are generally taken from several such fields. Not that Cournot avoids the use of deductive procedure, he makes his main points in the manner outlined and then proceeds to work out their implications.

Cournot is at his best and most instructive when discussing and illustrating anything related to the natural and deductive sciences. Students of the philosophy of science and scientific method should find a good deal of interesting material in the book. What he has to say about psychology and its place among the sciences deserves a place among the more noteworthy historical writings on psychology which, so far as I know, has not yet been accorded. Although science and philosophy should not be confused Cournot thinks they should not be divorced. Philosophising is likely to be most fruitful when associated with the main fields of human activity, especially with those concerned with discovering the laws and principles of nature. Cournot is at his worst when discussing morality ; when so doing he has a tendency to substitute eloquence for analysis and argument. He has little to say about theology. God is mentioned from time to time but whether in accents of conventional respect or genuine piety is not easy to decide. At least he never resorts to the Deity in order to sanctify an intellectual mare's nest or to support otherwise insupportable conclusions.

As for Cournot's philosophical calibre, the book itself bears ample witness to it. It is a pity that he has been neglected for such a long time and has had to wait so long before being translated into English. Our thanks are due to Professor Moore for doing this and for the care and scholarship which he has brought to bear upon his task. He has provided useful footnotes and an extensive bibliography ; the index is adequate. Professor Moore's lengthy introduction while informative and helpful will, I think, be found very difficult at times by most British readers.

H. HUDSON

The Nature of Metaphysics. Edited D. F. PEARS. Macmillan and Co. Ltd., 1957. Pp. vi + 164.

THIS is another volume of B.B.C. Third Programme talks. It contains an introductory discussion by H. P. Grice, D. F. Pears, and P. F. Strawson, which for this volume has been rewritten as a continuous essay; a final summing-up by G. Ryle, Mary Warnock and A. M. Quinton; and, in between, essays by S. N. Hampshire ("Metaphysical Systems"), B. A. O. Williams ("Metaphysical Arguments"), G. Buchdahl ("Science and Metaphysics"), P. L. Gardiner ("Metaphysics and History"), Iris Murdoch ("Metaphysics and Ethics"), and G. J. Warnock ("Criticism of Metaphysics").

No very clear answer emerges to the central question: what is metaphysics? More accurately, too many answers are given. Metaphysicians, we are told, have ontologised ("essayed to prove assertions of existence from conceptual considerations" instead of by experiment and observation). They have attempted to give a general account of the universe by deduction from a few first principles. Both these activities, it is agreed, are to be discouraged. But the metaphysician has also produced illuminating paradoxes (qualified approval); and he has engaged in conceptual investigations (as we all do), and conceptual revision (which at least some of us do some of the time, and which anyway is not altogether a bad thing).

Part of the difficulty about deciding just what "metaphysics" is to refer to undoubtedly derives from the fact that the word still has rather a smell clinging to it. We have, we like to think, thrown off the crudities of the earlier logical positivist dismissal of metaphysics, but enough of the emotional flavour of those days survives to make us uneasy about describing as metaphysics anything we are otherwise disposed to approve. Some have passed through this stage, and for them the word "metaphysics" is now quite disinfected. Thus at least one contemporary Oxford philosopher told this reviewer that he was perfectly happy to describe as "metaphysics" the activities of himself and his colleagues, and stressed the similarity between what goes on these days and what philosophers have always done. To take this line is to settle for that view of the metaphysician which sees him as an investigator and reviser of concepts. It is symptomatic of the liberalism of contemporary linguistic philosophy that this is the view of metaphysics most contributors to this volume are anxious to stress. "It is supremely a kind of conceptual revision which the metaphysician undertakes, a re-drawing of the map of thought—or parts of it—on new plan." It is left for the veteran of the earlier battles, Ryle, to insist: "If he is not an ontologist he is not a metaphysician". Ryle, if I read him rightly, grants that the metaphysician engages in conceptual revision, but sees this as secondary to his ontology and system building.

Ryle's fellow-contributors to the final discussion give general support to his insistence on this feature of metaphysics, yet it remains true that, this final discussion apart, the emphasis is on the metaphysician as conceptual reviser rather than as system builder. The trouble with this emphasis is not only that it picks out a derivative, rather than the basic, activity of those philosophers whom we should all agree to class as metaphysicians, but that in a laudable desire to be hospitable to our predecessors it distorts the account of their procedure by assimilating it too closely to what philosophers do now. One result of this is a tendency to call every philosophical question, doubt or problem, a metaphysical question, doubt or problem, so that no purpose is served by "metaphysical" which

"philosophical" does not serve. It is surely better to restrict "metaphysics" so that it is not coextensive with "philosophy", and the obvious way to do this is to save the word for the activity of producing or pretending to produce demonstrable systematic ontology. Metaphysicians have certainly done other things, among them being conceptual investigation and revision, but it is not these other things which make them metaphysicians.

It is generally agreed by the philosophers represented in this book that for metaphysics in this restricted sense there is, and should be, no future. This is true even of Miss Murdoch, in spite of her desire to convict contemporary moral philosophers of ignoring the connexions between morality and metaphysical views. This rejection of deductive metaphysics is based partly on verificationist arguments, but there is general consent to the view that these are too brusque to be convincing by themselves, and need supplementing by detailed analysis and examination of metaphysical arguments as they arise. Warnock clearly expresses this consensus of opinion when he praises Kant as a critic of metaphysics in preference to the logical positivists.

In humble mood, those taking part in the final discussion ask themselves what contemporary philosophers (meaning Oxford ones) fail to do which metaphysical philosophers have done, and how far these sins of omission should be remedied. The failure to provide any general account, the departmentalization of philosophy, is excused on the grounds of the comparative newness of the new philosophical techniques. More specific failures are noted, without any great promise of reform to come. Philosophical theology, one of the fields admitted to be largely untilled, has since these talks were first composed shown signs of new growth. Another, political philosophy, is still neglected, and I don't think Quinton's offer of work in the philosophy of law or in the methodology of the social sciences can be accepted as a satisfactory substitute. One thing is clear: any attempt to remedy these omissions which these present contributors can foresee will not take the form of a return to the discredited activity of building metaphysical systems.

I have preferred to confine this review to general remarks. It would not I think be profitable to discuss in detail the individual contributions. They are workmanlike, but they are clearly broadcast talks, not philosophical essays, and their publication in book form must be justified by their possible appeal to the educated public rather than by their interest for professional philosophers. I found Miss Murdoch's contribution the most thought-provoking, but it does not contain anything which she has not said as well and at greater length elsewhere. She has interesting things to say, but I should be surprised if when all the smoke has cleared away she will have made any serious breaches in the position of those (nowadays called the Oxford moralists) whom she criticizes. Some hard decisions are needed here, in particular as to how far the moral philosopher is to engage in a mere reporting of the way moral language is used, and how far he is to be concerned with giving a correct account of the facts which the use of moral language illuminates or fails to illuminate. Failure to get this clear is a besetting sin of contemporary moral philosophers, and it alone can explain why Miss Murdoch should accept the claim of her opponents that the contemporary analysis of moral language resolves the conflict between objectivism and subjectivism. It resolves the conflict only by the unadmitted rejection of objectivism.

R. G. DURRANT

The Beautiful, the Sublime and the Picturesque in Eighteenth-Century British Aesthetic Theory. By WALTER JOHN HIPPLE, Jr. Carbondale : Southern Illinois University Press, 1957. Pp. vi + 390. \$7.

PEOPLE have never talked of "the British aestheticians" in the way they have talked of "the British moralists"; and yet the debate about the sublime, the beautiful and the picturesque was quite as lively as the parallel one about duty and interest, reason and feeling, and was carried on by some of the same writers. A clear account of the issues raised and the arguments used should, then, be welcome.

But this is not quite what Professor Hippel has given us. He tells us that there are three ways in which a book of this kind may be written : "It can be handled as a philological inquiry, with the influence of philosophical and methodological principles minimized ; it can be composed dialectically, previous theories . . . being examined in the light of a schematism, a superior theory, provided by the historian ; finally, a closely literal survey of the arguments of conflicting theoreticians can be written, with attention directed upon philosophical issues when these are important, but without the superimposition of a more comprehensive theory of the analyst upon the theories which are his subject" (p. 188). Hippel has chosen the third of these ; which means in practice that he takes sixteen writers (Addison, Hutcheson, Hume, Hogarth, Alexander Gerard, Burke, Lord Kames, Hugh Blair, Reynolds, Reid, Alison, William Gilpin, Uvedale Price, Humphry Repton, Richard Payne Knight and Dugald Stewart) and summarises their views, one by one. "Philosophical issues" are "important" for him when it is necessary to understand them to appreciate an author's point. Consequently he is admirable on questions of origin and influence, and he has a good deal to say on method—though even here he sometimes confines himself to telling us when Burke is using Mill's Inverse Deductive Method (p. 85) or Alison the Method of Concomitant Variations (p. 165)—but he is usually careful to refrain from pointing out the flaws in an argument or from assessing the final value of an author's contribution to aesthetic theory. The result is a scholarly work which will be of value chiefly to specialists in the eighteenth century. The reader interested in the more general questions of aesthetics will find material here, but he will need to sift it for himself.

In his final chapter Hippel suggests that the "empiricism" of these writers consists in their resolute attempts to find psychological explanations for what "the rationalists take as primary givens" (p. 306). Modern British empiricists seem to have retreated from this position. The modern objection to all forms of reductionism leads to the multiplication of new "primary givens". Most modern writers on ethics, for example, seem to agree with Kant that universalisability is an *ultimate* feature of moral principles, and do not seriously consider the attempt of such writers as Hobbes and Hume to explain why human beings should have been driven to regulate their lives by principles of just this kind.

Now eighteenth-century aesthetic theory did consist, very often, in trying to reduce aesthetic categories to something else, or in trying to reduce all aesthetic categories to one all-embracing formula. And, on Hippel's account, the results do not seem to have been particularly happy. To take one glaring example, Burke suggested that the sublime pleases because it tenses the nerves, the beautiful because it relaxes them. This theory, Hippel tells us, "was reckoned an absurdity even in the eighteenth century" (p. 92); but this did not prevent Uvedale Price from finding

room for the third category, the picturesque, by explaining that it gave pleasure by neither relaxing nor stretching the nerve fibres, but leaving them "at their natural tone". As Hippel asks, "how does this differ from no stimulus at all?" (p. 20%). Or consider the formula by which Hutcheson tries to account for every kind of beauty: "uniformity in variety". This can obviously be made to apply to anything at all, since any two objects are bound to have some resemblances and some differences. Most of the eighteenth-century writers do distinguish between different types of aesthetic effect (the sublime, the beautiful, the picturesque) but these categories are still too broad. Payne Knight seems to be right when he declares that "the word Beauty entirely changes its meaning . . . accordingly as it is applied to objects of the senses, the imagination, or the understanding" (p. 255); but even he goes on to find a single formula for *visual* beauty: "the pleasure of the eye is wholly in broken and gradated light and colour" (p. 258).

It is, then, encouraging (and, to a modern reader, rather startling) to find Dugald Stewart putting forward the following theory of meaning and applying it to "beauty": "I shall begin with supposing that the letters A, B, C, D, E denote a series of objects; that A possesses some one quality in common with B; B a quality in common with C . . . (et cetera); while, at the same time, no quality can be found which belongs in common to any *three* objects in the series. Is it not conceivable that the affinity between A and B may produce a transference of the name of the first to the second; and that . . . the same name may pass in succession from B to C; from C to D; and from D to E?" (p. 288). Neither Stewart nor Hippel distinguishes this view from the common eighteenth-century doctrine of association of ideas. Stewart says, for example, that "sublime" applies primarily to altitude and then, by transference, to anything associated with altitude, even metaphorically. On this view, B, C, D, and E would each seem to have an "affinity" with A, though a different one in each case. But I suppose it might be retorted that associations may be "near" or "remote"; and to say that C is remotely associated with A may be merely to say that C is associated with B which is associated with A.

The associative theories, as worked out by Stewart, Alison and others, do avoid the more obvious absurdities which come from trying to force every kind of beauty into the framework of a single formula, but even these theories are often made plausible only by some distortion of the facts for which they are supposed to account. Moreover, the associationists also fall into the opposite kind of error: they cannot distinguish between "genuine" and "adventitious" associations. If we say that anything is beautiful which can be associated (however remotely) with some primary type of beauty, how do we distinguish between Shakespeare and Martin Tupper, between a daub and an Old Master? Most of these writers try to avoid the difficulty by distinguishing between the "natural" associations which all men form and the adventitious ones which are confined to a few. Hippel makes the point that this distinction enables them to avoid subjectivism; but he does not ask whether it really enables them to distinguish between good and bad art.

One is driven to the trite conclusion that writers on aesthetics, in the eighteenth as in other centuries, are illuminating when they discuss the details of some particular art (heroic poetry or landscape gardening) but either vacuous or patently inaccurate when they frame general theories about "the Beautiful". But is it quite certain, all the same, that there

are no discoveries, psychological or other, to be made about aesthetics in general?

D. H. MONRO

Traité de l'Argumentation. By CH. PERELMAN and L. OLBRECHTS-TYTECA. Presses Universitaires de France, 1958. 2 vols. Pp. 734. 1.200 fr.

IN modern times the procedures by which conclusions are established in formal logic, mathematics and the experimental sciences have often enough been studied by philosophers; other techniques of argument, or of persuasive discourse in general, have received little or no systematic attention. It is these other techniques which form the subject of this book: the authors describe their aim as 'l'étude des techniques discursives permettant de provoquer ou d'accroître l'adhésion des esprits aux thèses qu'on présente à leur assentiment'. This field of study is enormously wide; for, as the authors point out, constraining arguments have little place in human affairs, where nevertheless rational persuasion and reasonable choice have a large place. The field is wider, indeed, than the author's phrase, 'la théorie de l'argumentation', suggests; for they are in fact concerned with all discursive means of securing adherence to theses or points of view or ways of looking at things, whether or not such theses or views are presented as the conclusions of arguments. 'Discursive means' is indeed interpreted as stopping short of the mere expression of threats or offering of bribes; but with such obvious reservations as these there is almost no feature of language, or of conceptual thought in general, which may not find a place as a factor in discursive persuasion.

The handling of this diverse and manifold mass of material, of techniques, devices, forms of thought and expression, obviously presents a great problem. The authors classify, describe and trace connections with a will—and also with skill and lucidity. If their classifications and modes of presentation are not equally acceptable to all *auditoires particuliers*, this is not a fact which will surprise or disquiet them. I shall not attempt to summarise their treatment of a terrain of which they themselves say: 'Loin d'en avoir épousé la matière, nous en avons à peine entamé, et parfois même signalé, la richesse.' To indicate the range and diversity of topics discussed, I list a small random selection: the utility of a neutral style in preaching a novel morality; the use of the impersonal 'on' instead of the personal pronoun; the employment of co-ordinating conjunctions; metaphor, and figures of speech in general; the use of what are formally tautologies and contradictions; the type of argument known to members of committees as the argument of the slippery slope (*pente savonneuse*); the interactions between our assessments of people and their acts, groups and their members, historical periods and what falls within them; the nature of analogy; the antithesis between appearance and reality; the difference between differences of kind and differences of degree. The discussion of such 'factors of discursive persuasion' is illustrated throughout with admirable and often amusing examples. One has finally the impression that, apart from the more plainly utilitarian communications on the one hand, and *exposés* belonging to the more developed sciences on the other, there is little use of language which does not fall within the sphere of this study. Certainly all that is most humanly *passionnant* belongs to it.

Humane and anti-dogmatic in tone, relativistic and anti-parochial in tendency, this is an admirably civilised book. It is quite free both from tiresome obscurity and from still more tiresome polemics ; and it is very elegantly written.

P. F. STRAWSON

Mathematical Interpretation of Formal Systems. By TH. SKOLEM, G. HASENJAEGER, G. KREISEL, A. ROBINSON, HAO WANG, L. HENKIN, J. LOS. Studies in Logic and the Foundations of Mathematics. North-Holland Publishing Co., Amsterdam, 1955. 24s.

THOSE of us who picked up our elements of mathematical philosophy from Russell's *Introduction* to the same will remember his listing of Peano's postulates for arithmetic and then showing that these would still hold if the undefined terms 'O', 'number' and 'successor' were not understood in the usual way but some other (e.g. 'number' as even number and 'successor' as the result of adding two). Russell attempts to preclude such unintended interpretations by defining the arithmetical primitives in terms of logical ones ; but of course these could be given unintended interpretations too. Could we preclude them by adding to our postulates until we have a set so explicit that they will apply to the 'natural' number series and to nothing else ? Skolem found many years back that the answer to this is No, and sketches his proof in the first paper in this collection. The remaining papers mostly take their start from this result.

These papers—and Skolem's too—are technical contributions to 'metamathematics', originally delivered at a conference of workers in this field ; they are not addressed to philosophers. But there is obviously a philosophical discussion that Skolem's result starts up also. Does it mean that no finite set of postulates can yield all truths about natural numbers, and so refute the view that truth in arithmetic is simply provability from postulates ? I doubt it ; but it does at least mean that given a certain set of postulates which would be generally regarded as applying to the natural numbers, or any set including these, there can be no such thing as 'the' set of objects satisfying these postulates.

One detail worth commending to other readers with a philosopher's rather than a mathematician's logic is Skolem's ingenious procedure (pp. 2-4) for reducing all truth-functional and quantificational complexes of numerical equations to straight equations. The nerve of the final step is the mutual implication of 'For some number x , $f(x)$ ' and ' f (the least number such that $f(x)$)'.

A. N. PRIOR

Nomological Statements and Admissible Operations. By HANS REICHENBACH. Studies in Logic and the Foundations of Mathematics. North-Holland Publishing Co., Amsterdam, 1954.

I SUSPECT that if you found (however this is done) a *bona fide* man-in-the-street and asked him what followed from an impossible supposition, he would not answer 'Everything !' (with Lewis), nor would he answer (with Strawson) 'The question of what follows only arises when the supposition is possible'. What he would really like to say, if only the warring logicians would let him, is that from a given impossible supposition, as from a possible one, some things follow and some do not. For example,

from ' $2 = 3$ ' we can fairly infer ' $1 = 2$ ' (because if the successors of x and y are equal, so are x and y themselves), but not 'The sky is a limpet'.

Is this a policy that can be carried through consistently? It is to this and similar problems that Reichenbach here addresses himself, and I think his book shows that (i) the policy mentioned can be carried out; but that (ii) it's fiendishly complicated, and (iii) it can only be done by outraging the man-in-the-street's prejudices at some other point. Suppose we say, e.g. that 'It is and isn't wet' entails 'It isn't wet' because 'If both p and q then q ' is a principle which (a) holds whether the antecedent is possible or not, and (b) has instances in which its antecedent is possible; but the same contradiction does not entail 'It's Friday' because there is no principle with properties (a) and (b) that this one would exemplify. This (which isn't quite what Reichenbach would say about this example, but illustrates his general type of approach) is an attractive solution, but note firstly that it makes entailment non-transitive. For 'It is and isn't wet' entails (in this sense) 'It is wet and it either isn't wet or is Friday', by the respectable principle 'If both p and q , then both p and either q -or- r '; this in turn entails 'It's Friday', by the equally respectable principle 'If p , and either not- p or q , then q '; but by no respectable, i.e. (a)-(b), principle does 'It is and isn't wet' entail 'It's Friday' (cf. p. 80). Further, the (a)-(b) test above is too easy; it lets through, e.g. the entailment of 'It's either Friday or my birthday' by 'It's either both-wet-and-not, or my birthday'; for the antecedent of 'If either p or q -and-not- q then either p or r ' may be possible even if it has a disjunct that isn't.

Reichenbach had a wonderful eye for loopholes of this sort (helped, one gathers, by the eyes of his friends), and his elaborate devices for stopping them up command one's admiration even when they fail to gain one's assent.

A. N. PRIOR

Introduction à la Logique Contemporaine. By R. BLANCHÉ. Paris
Librairie Armand Colin, 1957. Pp. 208.

THIS book forms a most useful contribution to the French logical scene. Though it contains no novelties, and is throughout at a very elementary level, what it does say is said with clarity and a minimum of philosophical obfuscation. There is an introductory chapter on the purpose of symbolic logic, followed by chapters on the classical propositional calculus, non-classical calculi (modal logics, many-valued logics, and the intuitionist propositional calculus), the predicate calculus, and the logic of classes and relations. Blanché takes the traditional syllogism for granted, and in general aims to show how much more precise and comprehensive a tool modern symbolic logic is: this particularly emerges in the chapter on the predicate calculus, where the existential analysis is favourably contrasted with the more limited traditional scheme. Unfortunately, the book contains a series of minor misstatements (e.g. the hopelessly inadequate version of the substitution rule on p. 74). The book is far more a manifesto than a manual: there are virtually no proofs, no exercises, very few examples of rigorous reasoning, though much talk about it. Blanché should whet the appetite for formal logic, but in no degree will satisfy it. In this connexion the suggestions for further reading are slender, and the book ought to contain far more references to fuller discussions of points raised.

E. J. LEMMON

The Physical World of the Greeks. By S. SAMBURSKY. Translated from the Hebrew by MERTON DAGUT. London : Routledge & Kegan Paul, 1956. Pp. 255. 25s.

THIS book is not intended to be a general history of Greek Science. The author, who is himself a physicist, has concentrated his attention upon the achievements and limitations of the Greeks in the spheres of mathematics, astronomy and physics, wherein, as he says, their main contribution was made. He covers a very wide span, namely from the speculations of the Milesians in the sixth century B.C. to those of Ptolemy in the second century A.D.

There are two significant features of the book, both of which seem to be thoroughly commendable. First, the author's account is illustrated by extensive and very illuminating quotations from the writings of Greek and Roman philosophers and men of science. Hence the non-specialist reader is able to decide for himself to what extent, if any, the author is guilty of foisting upon the Greeks categories and concepts which are mere projections of modern thought. The second feature is that Professor Sambursky is throughout his book attempting to relate the Greek achievement to modern scientific thought, bringing out both the similarities and the divergences between the approach of the Greeks and that of modern science. Further, he is not content merely to record the scientific successes of the Ancient World; he everywhere tries to make clear the varying conceptions of scientific method of the Greeks and their philosophical presuppositions. It must, however, be added that the author is least successful in this last department ; for example, in his account of the Greek conceptions of knowledge and sensation or the distinction between primary and secondary qualities, topics which are treated in a misleadingly superficial way.

The main topics which are discussed at length are the development of the Greek conception of matter, motion and number, the cosmology of Aristotle, the atomic doctrines of Democritus, and particular attention is given to the work of the Stoics such as Chrysippus and Poseidonius. In spite of the fact that the terminology which he uses tends to be that appropriate to modern science, the author has avoided many of the pitfalls which face anyone who attempts to evaluate the work of an earlier age in the language of a later one.

The final chapter is an interesting discussion of the limitations of Greek physical science, with special reference to the poverty of its technical achievements, the lack of systematic experimentation and the comparative failure to apply the methods of mathematics to the solution of problems in natural science.

J. L. EVANS

Thinking towards Religion. By PAUL ROUBICZEK. Darwen Finlayson Ltd., London, 1957. Pp. 192. 15s.

THE question with which this book starts is : ' Faith in God is desirable, but is it intellectually honest ? ' By making a radical distinction between the world of science (external reality) and that of persons (internal reality), by arguing that feeling is an organ of knowledge, by connecting feeling with evaluation and by counting truth as a kind of value, the author attempts to show that ' truth needs personal participation ' (p. 148) and that the necessary limitation of scientific knowledge leaves room for faith.

Though 'there is no absolute knowledge in external reality', nevertheless 'we get glimpses of it in internal reality' (p. 162). Our thinking, which is in opposites, thus leads to religion, which also shares in this paradoxicality. A sample of the type of reasoning in this book : 'The theory (sc. of relativity) is based on an absolute, on the speed of light. . . There are, it is true, also attempts to abolish opposites; the theory of electrons dismisses matter by explaining it as energy, but then the electrons are gathered together again in quanta and fields; it is easier to renounce understanding than opposites. After all, these theories are based on mathematics, that is, numbers; and numbers are based on the opposition between the One and the Many . . .' (pp. 176-177).

The back dust-cover displays favourable comments by well-known people. Though works already published are often given similar advertisement, at least the comments by quoted reviewers are printed somewhere or other in full. Is it too much to protest at this growing habit of pre-selected pre-reviewing? (Though maybe there are exceptional cases: but is not a distinguished man's foreword more proper?)

R. N. SMART

The Philosophy of Value. By DEWITT H. PARKER. Michigan University Press (London : Oxford University Press), 1957. Pp. vi + 272. 40s.

DEWITT PARKER died in 1949, and this book has been edited from his manuscripts by William Frankena. To a reader not primarily educated in the tradition of Perry, the term 'value' which occurs in the title and recurs frequently in the course of the book might seem to fit rather precariously on to the substance of the discussion, which, in the main, offers an analysis of satisfaction. Parker would bridge the gap by denying that there is a gap to be bridged. Taking value as a generic concept, of which moral value, aesthetic value, economic value, etc., are species, and concentrating on intrinsic value, he claims that activities and experiences alone are, properly speaking, values; but these terms are not simply convertible, since some activities have minimal intrinsic value, and some are evil. Rather "an activity that is an enjoyment appears to be exactly what we mean by a value" (p. 7) or alternatively "we might perhaps define value as the objective of desire, as that which would satisfy desire" (p. 9). Apparent counter-examples that might tell against this identification of enjoyment and satisfaction of desire, e.g. the unexpected pleasure of the smell of flowers or the sound of music, are dealt with rather summarily. Ignoring any distinction between not wanting something and wanting not to have it, Parker claims that in most cases enjoyment does come from the assuagement of desire, and that we may therefore assume that this is true in all cases, and that uninvited pleasures are the assuagement of desires of the organism of which we are not aware. His definition of generic value, then, is in terms of either enjoyable activity or the assuagement of desire.

After a discussion of Some Rival Theories in chapter II and of the Expression of Value in chapter III, in which he argues that value statements are vectorial currents of feeling and as such neither true nor false, he turns to an analysis of values or satisfactions. The essential factors are he thinks, a desire, a goal or objective, a complementary object or means object, a judgement about the object's fitness for the realization of the goal, the assuagement of desire and anticipatory and memorial satisfactions. He also discusses the dimensions in which satisfactions are measured, of

which he thinks there are six: intensity, duration, volume (depending on complexity of desire), quality, height (or self-transcendence), and harmony (of desires).

In chapters V-VII he discusses the ways in which values or satisfactions are organized and evaluated by means of desires about desires. These higher order desires find expressions in standards, attainment of which brings its own satisfaction, falling short its own frustration. With reference to these standards subordinate desires are harmonized by renunciation, substitution, compromise or integration. Higher order desires also make up a person's life plan, the achievement of which is happiness.

Finally he turns to more specifically moral values. "The objective of morality" he says (p. 249) "may best be described as the preservation and fostering of a desired way of life for all the members of the group with whom identification is possible." One of the ways in which we attempt to preserve and foster a desired way of life is by making judgements of the form 'You ought not to do so and so'. The difference between moral and legal judgements of this form may be expressed, according to Parker (p. 252), by saying that the law says 'I want you not to do so and so', whereas the moralist says 'I want you not to want to do so and so'.

Some of the ways in which he analyses satisfaction, and discusses the subordination of one sort of desire to another, are not perhaps unsuggestive but they are not always easy to disentangle from the author's floridity of style. Of this book Frankena says that it "will seem like a rich garden" to many who find the fields of recent analytical philosophy somewhat sere." This is a matter of taste, and readers may perhaps choose to differ.

G. M. MATTHEWS

The Uses of Argument. By STEPHEN TOULMIN. Cambridge University Press, 1958. Pp. 259 22s. 6d.

THE aim of Professor Toulmin's book is to show that we must "reject as confused a conception of 'deductive inference' which many recent philosophers have accepted without hesitation as impeccable". It seems fair to add that the notion he rejects is the notion of deductive inference common to traditional and to mathematical logicians, who have merely widened its scope.

Professor Toulmin finds the ammunition for his attack on deductive inference in the writings of Wisdom, Ryle, Austin and others whom future historians of philosophy, rightly or wrongly, will consider as having been influenced by Wittgenstein. The book is thus of great interest both in itself and as a document in which some readers may find signs of a new development, particularly those who have found the originators of linguistic philosophy exciting and are now, perhaps, becoming a little bored by its more recent propagandists.

Toulmin wishes to develop a new—descriptive and comparative—theory of logic, a theory free from what he considers the misleading influence of certain inappropriate models—those of psychology and sociology and especially that of mathematics. The model which seems to him to be appropriate is jurisprudence, by which he means descriptive or, as it has sometimes been called, "positivistic" jurisprudence. Indeed, jurisprudence is for him not only a suitable model for logic but (almost) a species of it. Logic is (almost) "generalised jurisprudence". It would have been of great help to his readers here, if he had stated more clearly what he

considers jurisprudence to be ; and—which is still more important—in what precise respect jurisprudence is not a species of logic.

What he does say is this : Arguments are like law-suits, and just as it is the task of jurisprudence to characterize the essentials of the legal process, so it is the task of a properly conceived logic to characterize the essentials of the "rational" process, i.e. "the procedures and categories by using which claims-in-general can be argued and settled".

The similarity between logic and jurisprudence suggests to him the central distinction between field-invariant and field-dependent features of arguments. Consider, for example, the legal notion of "fact proven beyond reasonable doubt".

The force of this term is the same in civil as in criminal law, in that in either case a judge would consider a fact which has been so proven a basis for further deliberations. But the criteria by which one determines whether the term is applicable are different in the two cases—more rigorous proof being as a rule demanded in criminal cases than in civil. Toulmin suggests a similar distinction between the field-invariant force of modal terms ("possibly", "necessarily", etc.) and the field-dependent criteria of their applicability.

The "lay-out" of arguments in legal procedure and in logic, in his sense of "logic", is the same. We start with certain data *D* (e.g. Harry was born in Bermuda) and cross by means of an inference-warrant *W* (A man born in Bermuda will generally be a British subject) to the establishment of the claim *C* (Harry is a British subject). The warrant has as its backing *B*, the relevant statutes and legal provisions. As a rule the schema will also have to include exceptive and presumptive clauses, qualifications of the claim, etc. which we must, and here can, ignore. Toulmin remarks (p. 105) that whereas statements of warrants are "hypothetical, bridge-like statements", the backings for them "can be expressed in the form of categorical statements of fact quite as well as can the data appealed to in direct support of our conclusions". Indeed the model of positivistic jurisprudence demands that the adoption of the backing be a fact, just as it is fact that the Common Law is adopted in certain territories and other types of law in others. Just as in the case of legal claims, the claims depend for their special content on the context of the legal system, or of the part of it in which we argue, so in the case of other claims the claims depend on whether the field of argument is physics, ethics, and so forth.

Analytic arguments are taken by the author to be degenerate cases of substantial arguments, in the sense that only in them does "the backing for the warrant authorising it" include "explicitly or implicitly the information conveyed in the conclusion itself" (p. 125). It is worth remarking here, as indeed Toulmin does remark, that on this definition deductive are not the only analytic arguments.

The attack on deductive inference seems to be based on two assumptions—first what I should think the uncontroversial assumption that "the categories of formal logic were built up from a study of the analytic syllogism", and secondly on the assumption that "many of the paradoxical commonplaces of formal logic and epistemology spring from the misapplication of these categories to arguments of other sorts". In support of this last Toulmin gives extensive examples of what he considers such misapplication. A chapter—to this reviewer the least convincing of the book—is devoted to criticizing some current theories of probability. Another chapter finds the origins of epistemological theory in the view that substantial (i.e. non-analytic) arguments need to be redeemed. For Toulmin not only is this

a mistake but if logic and epistemology are to become fruitful "we must judge each field of substantial arguments by its own relevant standards" (p. 234).

Having now tried, on a plan perhaps not quite unoriginal in contemporary philosophical reviewing, first to convey some of the main contentions of this book, I ought next to proceed, however briefly, to some criticisms. First a minor one : in attacking deductive inference the author does not sufficiently allow for the elementary fact that in all fields of argument, including of course the field of law, deductive arguments—often of a highly complex kind—play a large role. This alone would suffice to justify the study of deductive arguments ; which is all that the logicians, whom Toulmin criticizes, pretend to do.

The second and, if it is just, more serious point will perhaps be most sharply brought out, after Professor Toulmin's own fashion, through an example. The inquisitors used the inference warrant (*W*) ' If anybody confesses (even under torture) to a crime, he must be deemed guilty of it'. The backing (*B*) was found in the relevant articles of that canon law which, to use the appropriate phraseology of positivistic jurisprudence, was *de jure* and *de facto* " adopted by the bulk of the population ". I have no doubt Toulmin would prefer to the warrants and backings of canon law the rather different warrants and backings of Common Law procedure. He would hardly justify his preference as one of taste, I think, and he might possibly modify his remarks on the relation between logic and jurisprudence in this matter. But I cannot find how he would deal with this central problem at all, from the point of view of his conception of logic. Lucidity in the author's argument here, does not match for me the lucidity of his style.

S. KÖRNER

A Source Book in Indian Philosophy. Edited by SARVEPALLI RADHAKRISHNAN and CHARLES A. MOORE. Princeton : Princeton University Press, 1957. Bombay : Oxford University Press. Pp. xxix + 684. 40s.

THIS is the first in a projected series of four source books in Oriental philosophy. The second, on Chinese philosophy, is scheduled for publication next ; and others will follow on Buddhist and Japanese philosophy, but the present volume already presents about 75 pages of Buddhist texts.

Here will be found, in context, the familiar gems of Hindu wisdom and technical Hindu philosophy : the beguiling Vedic hymn of creation (" Non-being then existed not nor being "), the revelation of the spiritual meaning of death by Yama the Prince of Death to Naciketas in the *Katha Upanishad*, the *tat twam asi* (" that art thou ") of the *Chandogya Upanishad*, the *neti, neti* (" God is not this, not that ") of the *Brihadaranyaka Upanishad*, the Buddha's fourfold truths of suffering and eightfold path of release from suffering, the *Gita*'s elaboration of the theme of inner-outer unity, the gnomic wisdom of the *Mahabharata*, the civil and criminal jurisprudence of *Manu*, the pithy *sutras* and wordy commentaries of the orthodox and heterodox systems, and—as a charming surprise—a section on contemporary thought, with selections from *Aurobindo's Life Divine* and Radhakrishnan's own *Idealist View of Life*.

The introductions to the various sections were prepared, with a few exceptions, by Radhakrishnan. They are adequate, but not commensurate with the polish, glow, and elevation of his other writings. Moore selected

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a mistake but if logic and epistemology are to become fruitful "we must judge each field of substantial arguments by its own relevant standards" (p. 234).

Having now tried, on a plan perhaps not quite unoriginal in contemporary philosophical reviewing, first to convey some of the main contentions of this book, I ought next to proceed, however briefly, to some criticisms. First a minor one : in attacking deductive inference the author does not sufficiently allow for the elementary fact that in all fields of argument, including of course the field of law, deductive arguments—often of a highly complex kind—play a large role. This alone would suffice to justify the study of deductive arguments ; which is all that the logicians, whom Toulmin criticizes, pretend to do.

The second and, if it is just, more serious point will perhaps be most sharply brought out, after Professor Toulmin's own fashion, through an example. The inquisitors used the inference warrant (*W*) 'If anybody confesses (even under torture) to a crime, he must be deemed guilty of it'. The backing (*B*) was found in the relevant articles of that canon law which, to use the appropriate phraseology of positivistic jurisprudence, was *de jure* and *de facto* "adopted by the bulk of the population". I have no doubt Toulmin would prefer to the warrants and backings of canon law the rather different warrants and backings of Common Law procedure. He would hardly justify his preference as one of taste, I think, and he might possibly modify his remarks on the relation between logic and jurisprudence in this matter. But I cannot find how he would deal with this central problem at all, from the point of view of his conception of logic. Lucidity in the author's argument here, does not match for me the lucidity of his style.

S. KÖRNER

A Source Book in Indian Philosophy. Edited by SARVEPALLI RADHAKRISHNAN and CHARLES A. MOORE. Princeton : Princeton University Press, 1957. Bombay : Oxford University Press. Pp. xxix + 684. 40s.

THIS is the first in a projected series of four source books in Oriental philosophy. The second, on Chinese philosophy, is scheduled for publication next ; and others will follow on Buddhist and Japanese philosophy ; but the present volume already presents about 75 pages of Buddhist texts.

Here will be found, in context, the familiar gems of Hindu wisdom and technical Hindu philosophy : the beguiling Vedic hymn of creation ("Non-being then existed not nor being"), the revelation of the spiritual meaning of death by Yama the Prince of Death to Naciketas in the *Katha Upanishad*, the *lat tvaṁ asi* ("that art thou") of the *Chandogya Upanishad*, the *neti, neti* ("God is not this, not that") of the *Brihadaranyaka Upanishad*, the Buddha's fourfold truths of suffering and eightfold path of release from suffering, the *Gita*'s elaboration of the theme of inner-outer unity, the gnomic wisdom of the *Mahabharata*, the civil and criminal jurisprudence of *Manu*, the pithy *sutras* and wordy commentaries of the orthodox and heterodox systems, and—as a charming surprise—a section on contemporary thought, with selections from *Aurobindo's Life Divine* and Radhakrishnan's own *Idealist View of Life*.

The introductions to the various sections were prepared, with a few exceptions, by Radhakrishnan. They are adequate, but not commensurate with the polish, glow, and elevation of his other writings. Moore selected

the individual texts presented here from existing translations. In many of these, felicity is sacrificed to scholarly impediments. In the Vaisesika Sutra, for example, we read : "The authoritativeness of the Veda (arises from its) being the Word of God [or being an expression of *dharma*]." Then, as if Pelion piled on Ossa—the bracketed exegesis and the parenthetical scilicet—were not enough, there is also a footnote at this point, which reads : "The text does not use the word 'God.' It uses 'tat' which means 'that'." To add the final straw, Moore says in his preface : "The use of parentheses and brackets is not altogether consistent."

Despite the minor editorial shortcomings, this volume is a useful source book for those who want to read not about Indian philosophy, but Indian philosophy itself.

WILLIAM GERBER

Parapsychology : Frontier Science of the Mind. By J. B. RHINE and J. G. PRATT. Oxford : Blackwell Scientific Publications, 1957, pp. ix + 220.

In the first part of this book the authors examine the findings of parapsychology and its relation to other branches of scientific inquiry. Psi phenomena do not fall within the scope of physical principles but they do appear to be conditioned to some extent by biological and psychological factors. For instance, in the Anderson-White research with public school students, children more often than not score positively when tested by teachers whom they like, whereas disliked teachers seem to stimulate negative deviations from chance expectancy. Similarly, in Schmeidler's tests, subjects who believe in the possibility of ESP tend to score positively and sceptics to have negative results.

The second part of the book is devoted to a thorough description of testing conditions and the assessment of results. The authors strongly recommend that experimenters test their hypotheses under exploratory conditions before investing in the experimental paraphernalia necessary for conclusive work. They also show how the subject's interest and spontaneity, vital for successful testing, can be maintained while the safety precautions are built up.

The strongest and best developed points in the book relate to experimental procedure, the weakest and least developed consist in the arguments regarding the logical implications of psi occurrences. The experimental sections, however, should be required reading for anyone who wants to carry out sound and sensible experiments in this field.

The book has an index, a glossary and the statistical formulae and tables needed for most experimental work. All chapters are followed by extensive bibliographies.

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